Déjà Vu? Short-term training in Germany 1980–1992 and 2000–2003

Fitzenberger, Bernd ; Orlanski, Olga ; Osikominu, Aderonke ; Paul, Marie

Abstract: Short-term training has recently become the largest active labor market program in Germany regarding the number of participants. Little is known about the effectiveness of different types of short-term training, particularly their long-run effects. This paper estimates the effects of short-term training programs in West Germany starting in the time periods 1980–1992 and 2000–2003 on the three outcomes employment, earnings, and participation in long-term training programs. We find that short-term training shows mostly persistently positive and often significant employment effects. Short-term training focusing on testing and monitoring search effort shows slightly smaller effects compared to the pure training variant. The lock-in periods lasted longer in the 1980s and 1990s compared to the early 2000s. Short-term training results in higher future participation in long-term training programs.

DOI: https://doi.org/10.1007/s00181-012-0590-4

Déjà Vu? Short–Term Training in Germany 1980–1992 and 2000–2003[1]

Bernd Fitzenberger,[♭] Olga Orlanski,[♮] Aderonke Osikominu,[♯] and Marie Paul[§]

[♭]Albert–Ludwigs–University Freiburg, ZEW, IZA, IFS, bernd.fitzenberger@vwl.uni-freiburg.de
[♮]Albert–Ludwigs–University Freiburg, olga.orlanski@vwl.uni-freiburg.de
[♯]University of Zurich, Albert–Ludwigs–University Freiburg, IZA, osikominu@vwl.uni-freiburg.de
[§]née Waller, University of Duisburg-Essen, marie.paul@uni-due.de
Correspondence:
Dept. of Economics, Albert–Ludwigs–University Freiburg, 79085 Freiburg, Germany

This version: January 16, 2012

Abstract: Short–term training has recently become the largest active labor market program in Germany regarding the number of participants. Little is known about the effectiveness of different types of short–term training, particularly their long–run effects. This paper estimates the effects of short–term training programs in West Germany starting in the time periods 1980 to 1992 and 2000 to 2003 on the three outcomes employment, earnings, and participation in long–term training programs. We find that short–term training shows mostly persistently positive and often significant employment effects. Short–term training focusing on testing and monitoring search effort shows slightly smaller effects compared to the pure training variant. The lock–in periods lasted longer in the 1980s and 1990s compared to the early 2000s. Short–term training results in higher future participation in long–term training programs.

Keywords: short–term training, employment effects, future training participation, administrative data, active labor market programs

JEL: C 14, J 68, H 43

---

[1]This study is part of the two projects "On the effectiveness of further training programs. An evaluation based on register data provided by the Institute for Employment Research, IAB" ("Über die Wirksamkeit von Fortbildungs- und Umschulungsmaßnahmen. Ein Evaluationsversuch mit prozessproduzierten Daten aus dem IAB"; IAB project number 6–531A) and "Employment effects of further training programs 2000–2002 – An evaluation based on register data provided by the Institute of Employment Research, IAB ("Die Beschäftigungswirkungen der FbW–Maßnahmen 2000–2002 auf individueller Ebene – Eine Evaluation auf Basis der prozessproduzierten Daten des IAB"; IAB project number 6–531.1A). These projects are joint with the Swiss Institute for Empirical Economic Research at the University of St. Gallen (SEW) and the Institute for Employment Research (IAB). We gratefully acknowledge financial and material support by the IAB. We especially thank Stefan Bender for his efforts to make available the data on which this study is based. The usual caveat applies.

1 Introduction

> *"... there is almost never a stable set of active programmes to evaluate. Countries are continuously chopping and changing the mix of programmes."*
>
> *Martin and Grubb (2001, p. 21)*

Over the past few years, active labor market policies (ALMP) have placed a greater emphasis on job search assistance, monitoring and testing work availability, as well as limited training to activate the unemployed in the short run (OECD, 2007). There has been a greater focus on activating the unemployed to find unsubsidized jobs instead of placing the unemployed in traditional long–term training programs or public employment schemes. Short–term programs are replacing longer programs in order to prevent long lock–in effects.

Public sector sponsored training has traditionally been a main part of ALMP in many countries including Germany; see the surveys in Fay (1996), Martin and Grubb (2001), and Kluve (2010). Although there were many pessimistic assessments regarding the usefulness of such programs, these surveys point out that small scale training programs, which are well targeted to specific groups and which involve a strong on–the–job component, can show positive employment effects. Little is known in the literature about the medium– and long–run effects of activation strategies which combine training, job search assistance, and monitoring.[2] In Germany, the focus on activation strategies is reflected in the recent shift away from traditional longer further training programs, typically lasting a couple of months up to two years, to short–term training programs (*Trainingsmaßnahmen*, henceforth denoted by 'ST00'≡short–term training in the 2000s) lasting at most twelve weeks.

[2]There are two recent notable exceptions for the US. Dyke et al. (2006) divide welfare–to–work programs in Missouri and North Carolina into three categories defined by their intensity: first only assessment, second job readiness or job search activities, and third more intensive training (including for example basic education or vocational training). The study estimates the earnings effects of these programs over a period of four years. Heinrich et al. (2009) study the employment and earnings effects of participating in programs of different intensity as part of the Workforce Investment Act in different US states. The results of both studies suggest that the employment or earnings effects of short–term programs involving assessment or counseling are positive but fairly short–lived whereas more intensive training shows greater gains in the long run. Some features of these US programs, like for example an assessment of the opportunities of the job–seeker, are similar to the German short–term training programs considered here. Important conceptual differences are, first, that participants in short–term training in Germany have to be unemployed to be eligible, whereas this is not the case in the US. Second, participation in German programs is often mandatory. In fact, often the reason for the assignment of short–term training is to check or monitor the willingness to work of a transfer recipient. In the US, job–seekers usually have to ask for training. Third, the available studies for the US do not analyze explicitly the effect of short–term training on future participation in long–term training programs. Assessment of the latter is a policy goal in Germany but not the US.

In fact, ST00 have become the largest program in Germany in terms of the number of participants with 1.07 Million individuals entering such programs in 2007 (Bundesagentur für Arbeit, 2007, pp. 54, 57). In contrast, only 356 thousand individuals entered longer further training programs in 2007.[3] Longer further training programs used to be the largest programs in Germany but have been replaced to a large extent by the much less costly ST00 programs. In light of the recent evidence that long training programs mostly show positive long–run employment effects (Fitzenberger et al., 2008; Lechner et al., 2011), one might be concerned that a focus on activation strategies comes at the expense of pushing the unemployed into unstable jobs which do not result in permanently better employment prospects. There are different types of ST00 programs. In this paper, we distinguish programs which focus on skill provision and programs which focus on testing and monitoring search effort.

Between 1980 and 1992, short–term training programs similar in nature to ST00 were in place in West Germany (and since 1990 also in East Germany). These were the 'Programs According to Article 41a Employment Promotion Act' (*Maßnahmen nach §41a Arbeitsförderungsgesetz*, henceforth denoted by 'ST8092'≡short–term training between 1980 and 1992). Due to budgetary reasons these programs were abolished in 1992. In 1998, short–term training in the form of ST00 was reintroduced into the Social Code III (*Sozialgesetzbuch III*) that currently regulates labor market policy. While activation and monitoring are major goals of ST00, the older ST8092 focus solely on job search assistance, limited training, and guidance towards future participation in long–term training programs. Furthermore, the ST8092 programs were targeted to the low–skilled and hard–to–place unemployed. The common features of the two programs are provision of short–term training, assessment of the unemployed (e.g. regarding future assignment to longer labor market programs), and job search assistance.

There have been a number of studies estimating the effects of short–term training since 2000 (ST00) applying different program evaluation estimators (Hujer et al., 2006; Wunsch and Lechner, 2008; Biewen et al., 2007; Stephan et al., 2006; Büttner, 2008; Osikominu, 2009) using rich administrative data and state–of–the–art econometric methods. We are not aware of any study which uses modern approaches to estimate treatment effects for the older ST8092 programs. In the following, we summarize the evidence for ST00 and other short training programs in West Germany. Lechner et al. (2011) analyze shorter further training programs in the 1990s that last longer than ST00 or ST8092 programs and provide more sizeable investments

[3] In 2000, there was a reverse ranking with 552 thousand individuals entering longer further training programs and 477 thousand individuals entering ST00 (table 2).

into occupational skills. This study finds that the cumulative long–run employment effects of shorter training are higher than for longer training programs. Lechner and Wunsch (2009) show that the effect of longer training programs differs over the business cycle such that these programs show better partial equilibrium employment effects when unemployment is high. This is both due to less pronounced negative lock–in effects in the short–run and higher long–run positive program effects. This suggests that the activation effect of these programs on the unemployed is stronger in times of high unemployment. To our knowledge, no comparable evidence exists for short–term training with its strong focus on activation. Note that differences in lock–in effects are likely to be of less relevance for short–term training because of its shorter duration.

Biewen et al. (2007) and Wunsch and Lechner (2008) show that for the early 2000s ST00 tends to perform better than longer training programs regarding their employment effects in the first two to three years after program start. Biewen et al. (2007) find some significantly positive employment effects for ST00 in West Germany, whereas Wunsch and Lechner (2008) find no significantly positive treatment effects. Both studies use a matching approach relying on the conditional independence assumption. Wunsch and Lechner (2008) use a static evaluation approach.[4] They match participants and non–participants based on participation status during the observed time period to estimate the effect of treatment versus no–treatment. The temporal alignment during the unemployment spell occurs by drawing random program starts for the non–participants. For the German institutional setting, such a static definition of the treatment and control groups likely leads to a downward bias in the estimated treatment effect. If one defines those as treated who receive treatment during the observed time period and those as untreated who do not receive treatment during this time period, one effectively conditions on future outcomes. Since participation is only possible as long as an individual remains unemployed, unlucky job–seekers who did not manage to find a job quickly are overrepresented in the treatment group and lucky job–seekers who quickly found a job are overrepresented in the non–treatment group. This leads to a downward bias of the static estimator (Fredriksson and Johansson, 2008). Biewen et al. (2007) argue that the effect of treatment versus no–treatment cannot be estimated in a dynamic setting using matching and therefore focus on the effect of treatment versus waiting building on the approach suggested by Sianesi (2004, 2008) in a similar context. We use the same approach as Biewen et al. (2007) in this paper. This approach stratifies the sample according to the different treatment starting dates observed. It then defines treatment effects conditional on remaining unemployed until a given date and uses

[4]The same framework is also used in Lechner and Wunsch (2009) and Lechner et al. (2011).

for matching all non–participants who are still unemployed at that date, irrespective of whether they participate at a future point in time.

Hujer et al. (2006) and Osikominu (2009) use a multivariate continuous–time duration framework in the spirit of Abbring and van den Berg (2003) to estimate the effects of ST00 on the hazard rates out of unemployment and out of subsequent employment.[5] The results of these studies are not directly comparable to studies that apply, as we do, sequential matching techniques. First, the two approaches estimate different treatment parameters. Sequential matching focuses on the effect of receiving treatment at a given elapsed unemployment duration in the population of survivors at the given date. The duration model framework, in contrast, proposes a model for the joint selection into outcome states and treatments over time, and therefore allows estimation of treatment effects that do not condition on survival in unemployment. Furthermore, the identification strategies of the two approaches differ. While matching techniques rely on a selection on observed variables strategy, the duration framework allows in addition for selection based on unobserved variables that are separable from the observed components in the hazard rates. Hujer et al. (2006) as well as Osikominu (2009) find that ST00 have a positive effect on the exit rate to work, thus reducing unemployment duration. Osikominu (2009) finds in addition that ST00 tend to reduce the exit rate from employment back to unemployment, but to a smaller degree than more comprehensive long–term training schemes. Hujer et al. (2006) find that low–skilled men tend to benefit more from ST00 than low–skilled women.

The studies reviewed so far do not distinguish between different types of ST00. Stephan et al. (2006) consider participation in different versions of ST00 in the second half of the year 2002. The study uses a matching estimator and finds differing results depending on the type of ST00. The monitoring and testing version of ST00 does not show positive results, whereas the training versions show significantly positive or negative results depending upon whether the training takes place in a firm. Stephan et al. (2006) – like Biewen et al. (2007) – estimate the effect of training versus waiting. Their outcome variable is the probability of remaining in registered unemployment whereas we consider as outcomes the monthly employment status, half–yearly earnings, and monthly enrolment in long–term training. As the focus of their paper is to introduce a new targeting system of the labor agency

[5]Hujer et al. (2006) analyze the duration of unemployment only, while Osikominu (2009) also considers the duration of subsequent employment spells. Hujer et al. (2006) only model the duration until entry into short–term training, while Osikominu (2009) models the competing risks of starting a short–term or long–term training program. Also, the observation period used by Osikominu is longer.

and to produce comparable results for very different active labor market programs, their estimation approach does not focus on producing unbiased estimates for ST00 programs in particular. Büttner (2008) uses a very small experimental data set, which comprises three different subsamples, i.e. part–time unemployed women, unemployed who previously worked in manufacturing, and young unemployed aged 20 to 27, from one local employment office in West Germany in 2005. The study uses an experiment to investigate the effects of an ST00 program that involves monitoring and testing (similar to the testing and monitoring variant in our paper) on the outcome variables exit from unemployment and entry into employment. Because of attrition and non-compliance after random assignment (among 189 unemployed receiving an invitation only 77 actually participate), the study applies propensity score matching to balance personal characteristics between treated and controls. The focus of the study is on distinguishing the effects of the announcement of treatment ('threat effect') from the effect of actual treatment. The study does not find any significant effects, neither for the announcement of treatment nor for the effect of actually participating. Most of the point estimates are close to zero.

The literature review reveals that most previous studies do not distinguish between different types of ST00 programs and that estimates of the long–run effects of short–term training are missing. This paper estimates the effects of short–term training programs in West Germany both for the time period 1980 to 1992 (ST8092) and 2000 to 2003 (ST00) for the outcomes employment, earnings, and participation in long–term training programs. This paper is the first to use state–of–the–art estimators of treatment effects for the short–term training programs in the 1980s and early 1990s. We investigate in particular whether there are lasting positive effects on employment outcomes and whether participation in these programs leads to higher participation in long–term training programs afterwards. We investigate whether treatment effects of ST8092 vary over calendar time. Furthermore, we analyze participation in short–term training in the early 2000s in an analogous way in order to compare the results for both periods. Because the ST8092 programs were not intended to test and monitor the unemployed, we distinguish two versions of ST00, namely the training variant, which focuses on skill provision (QST00) and the monitoring variant, which focuses on testing and monitoring search effort (MST00). We argue that the ST8092 programs are comparable to the QST00 version of ST00.

Methodologically, this paper follows the approach of Sianesi (2004) and estimates the effect of starting treatment after some given unemployment experience against the alternative of not starting treatment at this given point in time, based on the sample of individuals who are still unemployed at that given time. We use the same

methodological approach in all our subsamples in order to be able to compare the results for the 1980s, 1990s, and early 2000s. Most evaluation studies in the past used a static approach to evaluate the effects of receiving treatment against the alternative of not receiving treatment during a fixed observation window.[6] In a dynamic setting in which the job–seekers are continuously at risk of being assigned to a program as long as they remain unemployed the timing of events becomes important; see Abbring and van den Berg (2003), Fredriksson and Johansson (2008), and Sianesi (2004, 2008). Static treatment evaluations attempt to estimate the effect of receiving treatment versus not receiving treatment. However, in a dynamic setting this leads to biased treatment effects because the job–seekers with shorter unemployment durations are less likely to be in the treatment group. Thus, the static approach implicitly conditions on future outcomes (Fredriksson and Johansson, 2008). The sequential matching approach suggested by Sianesi (2004) avoids this problem but does not identify the effect of treatment versus no–treatment; instead, it identifies the effect of treatment at a given point in time versus no–treatment at that point in time, which implies the possibility of receiving treatment at some later point in time (i.e. the controls wait and search further in open unemployment).

Appropriate data for a long–term evaluation of public sector sponsored training programs were not available for a long time. This is the first paper using administrative data covering such a long time period, namely 18 years in the 1980s and 1990s and four years in the early 2000s to study the medium–term and, for the earlier time period, also the long–term employment effects of short–term training. The comparison between the earlier and the more recent time period is interesting because of the similarities between the two programs. In addition to employment and earnings, we also consider the effects on future participation in longer further training programs. This is important because one stated goal of short–term training in Germany is to assess the unemployed's need to participate in longer–term training programs. However, with an increasing focus on short–run activation strategies this goal may have declined in importance over time.

According to our results, short–term training shows mostly persistently positive and often significant employment effects. The effects are particularly strong for those participants who receive training during months seven to twelve of the unemployment spell. The effects for short–term training starting during the second year of the unemployment spell tend to be smaller.[7] The monitoring variant MST00 shows

[6]Biewen et al. (2007) is an exception as well as Hujer et al. (2006) and Osikominu (2009) who use a different dynamic approach than ours.

[7]Note that this result does not imply that moving the start of training for those who receive training late in their unemployment spell to an earlier time period would necessarily increase their

slightly smaller effects compared to the pure training variant QST00. The lock–in periods last longer for ST8092 compared to ST00 and the employment effects tend to be smaller for the earlier time period compared to QST00 but not compared to MST00. The earnings effects follow a similar pattern as the employment effects. Short–term training results in higher future participation in longer further training programs and this effect is much stronger for ST8092 and MST00 than for QST00. Note that our results only provide partial equilibrium estimates.

The remainder of this paper is structured as follows: Section 2 discusses the institutional aspects of short–term training in Germany. Section 3 presents the data used. Section 4 describes the methodological approach to estimate the treatment effects. The empirical results are discussed in section 5. Section 6 concludes. The appendix provides detailed empirical results. An on–line appendix with further detailed information about the data and further detailed results is published on the website of the journal together with the online publication of this article.

2 Institutional Background

In Germany, training is traditionally a very important part of active labor market policy that aims at permanently reintegrating unemployed individuals into the labor market.[8] Among the different types of training programs offered, long–term further training programs with a duration of up to two years used to play the most important role since their introduction in 1969. During the 1980s and since 1999, short–term training programs have been used on a large scale, too. Table 1 displays the entries into different types of active labor market programs in West Germany in the period 1979 to 1992. It can be seen that entries into short–term training rose steadily until 1987, remained at a lower level in 1988 and 1989 and peaked again in 1990. Table 2 shows the participation numbers in Germany as well as West Germany for the more recent period since 1999. In recent years participation in short–term training has risen considerably. Since 2001, short–term training has become the most important type of training in terms of the number of participants.

— Insert tables 1 and 2 about here —

employment gain from training. The groups of participants at different points in time differ due to the dynamic selection and it may be the case that a smaller impact of training for the very long–term unemployed is due to the selection into long–term unemployment and not to the timing of training.

[8] Other important policy instruments are for instance employment subsidies, job creation in the public sector, and schemes to promote self–employment.

Modern short–term training programs (ST00) have two main goals. First, they are supposed to enhance reintegration of the participants into employment through guidance and qualification. This may comprise training in job search skills through activities such as job–application training, simulation of job interviews or general counseling on job search methods. It may also involve the provision of specific skills (like limited computer skills or some technical tasks) that are necessary to improve the job–seeker's labor market prospects.[9] The second aim of short–term training is to assess the job–seekers' labor market opportunities and their suitability for different jobs but also their availability and willingness to work. This may entail the preparation of detailed work plans to reintegrate the job–seeker into the labor market which can include participation in a long–term training program.[10] The availability of the unemployed is checked by pledging him or her to attend the full–time training program. In our empirical analysis we therefore distinguish short–term training programs for which the objective of qualifying the job–seeker dominates from programs that put more emphasis on testing the availability for work and assessing the job–seekers' opportunities using the information on the program codes in the data. Such a distinction can only be an approximation, as the same program can serve both purposes, even for the same participant. However, this distinction is also useful for the comparison with short–term training in the period 1980 to 1992, where testing work availability was not an (official) goal.

ST00 programs last between two and twelve weeks (with median duration around four weeks). Therefore, they are relatively cheap compared to the longer further training programs. In fact, a one–month short–term training course costs on average €550 per participant, whereas participation costs for a further training course lasting nine months amount to about €5850; see Biewen et al. (2007, table 1).

In the 1980s and 1990s, there existed short–term training programs (ST8092) that were very similar to those described above. The law governing active labor market policy at that time, the Employment Promotion Act *(Arbeitsförderungsgesetz)*, included an article on 'Programs to Improve the Employment Chances of the Unemployed' (*Maßnahmen zur Verbesserung der Vermittlungsaussichten für Arbeitslose*). The number of this article in the Employment Promotion Act gave the programs their name: 'Programs According to Article 41a'. These programs where introduced in 1979 after the German labor market conditions had worsened in the 1970s and the number of long–term unemployed had risen considerably.

[9] For more details on the contents of short–term training see Kurtz (2003).

[10] One element of the law called Job–AQTIV Gesetz introduced in 2002 is to assess the job–seeker soon after becoming unemployed. This may be done through a short–term training program (Kurtz, 2003).

ST8092 programs were particularly targeted at individuals with lower re–employment chances such as women, individuals without formal qualification and the long–term unemployed. Hard–to–place and low–skilled individuals were under–represented in the existing long–term training programs. Short–term training was intended to counsel job–seekers about their employment chances and the possibilities of participating in long–term training programs on the one hand and to teach limited skills helpful for either employment or participation in a long–term training program on the other hand (Dobischat and Wassmann, 1981). Similar to ST00, ST8092 programs mostly consisted of full–time classroom training. The curriculum covered e.g. job counseling, information on public sponsored further training programs and on the general labor market situation, application and communication training, visiting firms and exercises with the intention of boosting the participants' self–confidence. The maximum length was in general six weeks and there was no exam at the end of the course (Schneider, 1981).

At the end of 1992, ST8092 programs were abolished in order to reduce the costs of active labor market policy in a time of narrow budgets. Short–term training programs were reintroduced in 1997 (in the first years with a small number of participants only) and gained importance from 1999 onwards (Kurtz, 2003).[11]

When becoming unemployed individuals have to personally register at the local labor office. This involves a first counseling interview with the caseworker. Further interviews may follow from time to time. Based on these interviews in general the caseworker decides whether to assign an unemployed person to a program. Besides being registered as unemployed or as a job–seeker at risk of becoming unemployed, candidates for short–term training do not have to fulfil any additional eligibility criteria. Depending on regional and local circumstances, caseworkers exercise a considerable amount of discretion when allocating the unemployed to the different programs. Suitable programs are chosen from a pool of public and private providers. Job–seekers have no legal right to claim program participation; rather, it is up to the caseworker to decide whether training is considered necessary and in addition there are often supply constraints for specific programs. Usually a caseworker will at least discuss the ideas and desires of the job–seeker with him or her. But in the end, the caseworker may, on the one hand, refuse to assign a program or, on the other hand, assign a program against the wishes of the job–seeker. The latter

[11] From 1993 to 1997, there existed no public sponsored short–term training programs, but in some cases job–seekers were allowed to participate in externally offered short qualification programs while receiving transfer payments. In principle, participants had to cover program costs themselves. But in many cases they received an allowance from the labor agency to cover at least part of the program costs (Kurtz, 2003).

applies in particular for MST00 programs. If the job–seeker refuses to attend the assigned program, he or she runs the risk of being sanctioned by losing his or her transfer payments for some weeks.

The employment office pays all direct training costs for short–term training programs. In addition, ST00 participants continue to receive unemployment benefits or means–tested unemployment assistance, if they are eligible for such transfer payments. Thus, in the early 2000s, there exist no pure financial incentives for unemployed individuals to participate in ST00, in contrast to the situation in Germany before 1998. In the 1980s, short–term training was treated in the same way as longer further training programs. This means that participants who fulfilled certain eligibility criteria (mainly 720 days of employment subject to social security contributions within the last three years) received an income maintenance allowance which was more generous than the usual unemployment compensation. Those who where not eligible to receive income maintenance allowance continued to receive the means–tested unemployment assistance (Bender et al., 2005).

3 Data

3.1 Administrative Data Sets Used

This study uses large administrative data sets for both time periods under investigation. For the 2000s, the empirical analysis is based on the so–called Integrated Employment Biographies Sample (IEBS), a data set which has recently been made available by the Federal Employment Office of Germany.[12] The IEBS consists of a 2.2% random sample of individual data drawn from the universe of data records collected in four different administrative processes: the Employment History (*Beschäftigten–Historik*), the Benefit Recipient History (*Leistungsempfänger–Historik*), the Data on Job Search Originating from the Applicants Pool Database (*Bewerberangebot*), and the Participants–in–Measures Data (*Maßnahme–Teilnehmer–Gesamtdatenbank*).[13]

The Employment History is based on social insurance register data comprising employment information for employees subject to contributions to the public social

[12]For detailed information on the IEBS see Hummel et al. (2005) and Bender, Biewen et al. (2005). Information in English can be found in Jacobebbinghaus and Seth (2007) or on the website of the Research Data Center (FDZ) of the German Federal Employment Office (http://fdz.iab.de/en.aspx).

[13]The data used here has been supplemented with some additional information that are not available in the standard version.

security system. It covers the time period from 1990 to 2004. The main feature of these data is detailed daily information on the employment of each recorded individual. We use this information to account for the labor market history of individuals as well as to measure employment and earnings outcomes. For each employment spell, in addition to start and end dates, data from the Employment History contains information on personal as well as job and firm characteristics such as the wage, industry, and occupation.

The Benefit Recipient History, the second data source, includes daily spells of unemployment benefit, unemployment assistance and income maintenance allowance payments individuals received between January 1990 and June 2005.[14] The Benefit Recipient History provides information on the periods in which individuals were out of employment and therefore not covered by the Employment History. Moreover, we use additional information contained in the Benefit Recipients History involving sanctions and periods of exclusion from benefit receipt that may serve as indicators of a lack of motivation. Based on the information in the Employment and Benefit Recipient Histories we calculate the individual entitlement periods to unemployment benefits.[15]

The third data source included in the IEBS is the so–called Data on Job Search Originating from the Applicants Pool Database, which contains rich information on individuals searching for jobs covering the period January 2000 to June 2005. The spells include detailed information concerning job search and personal characteristics, in particular on educational qualifications, nationality, and marital status. They also provide information on whether the applicant wishes to change occupation, how many job proposals he or she already got, and about health problems that might influence employment chances. Finally, the data on applicants include regional and local identifiers, which we use to link regional and local information, for example unemployment rates at the district level.

The Participants–in–Measures Data, the fourth data source, contains detailed information on participation in public sector sponsored labor market programs covering the period January 2000 to June 2005. The data consist of spells indicating the start and end dates at a daily level, the type of the program as well as additional

[14]Until the end of 2003, unemployed individuals in Germany (who had worked in a job subject to social security taxation before) received unemployment benefits for the length of the benefit entitlement period, which depends upon the length of prior employment. Afterwards, they received the means–tested unemployment assistance if eligible. The income maintenance allowance is a transfer payment participants of most types of training programs (but not participants of ST00) usually received instead of unemployment benefits to cover their living costs while in training.

[15]For this purpose we rely on Plaßmann (2002) who summarizes the regulations regarding entitlements to unemployment benefits.

information. The Data Base of Program Participants allows us not only to identify participation in short–term training, but also in other programs such as employment subsidies. This is useful, as it enables us to distinguish between regular and subsidized employment when evaluating employment and earnings outcomes.

For the earlier time period covering the 1980s and 1990s, we use administrative individual data from three different sources. These data were assembled for the purpose of evaluating public sector sponsored training programs, see Bender, Bergemann et al. (2005) for a detailed description. The first data source is the IAB Employment Subsample (*IAB Beschäftigtenstichprobe*, IABS) of the Institute for Employment Research (IAB), see Bender et al. (2000) and Bender, Bergemann et al. (2005, chapter 2.1). The IABS is a 1% random sample of all employment records liable to social insurance contributions in the period 1975–1997. It also contains information on periods with transfer payments from the unemployment insurance system. The second data source is the Benefit Payment Register (*Leistungsempfängerdatei*, LED) of the Federal Employment Office; see Bender, Bergemann et al. (2005, chapter 2.2). These data consist of spells of transfer payments granted to the unemployed and to program participants in the period 1975–1997. They include very detailed information about income maintenance payments, which allows identification of participation in different training programs, including the ST8092 programs investigated here. These benefit data contain more detailed information than the benefit data available in the IABS. The two data sources were merged to create the so–called IABS–LED data set, see Bender, Bergemann et al. (2005) for details. Based on the IABS–LED data we calculate the individual entitlement periods to unemployment benefits.

As a third data source, we use an administrative survey on training participation, the so called FuU–data, see Bender, Bergemann et al. (2005, chapter 2.3). The Federal Employment Office collected these data for all participants in further training, retraining, and other training programs for internal monitoring and statistical purposes. For every participant, the FuU–data contain detailed information about the program and the participant.

The FuU–data were merged with the combined IABS–LED data by social insurance number and additional covariates. Numerous corrections were implemented in order to improve the quality of the data, see Bender, Bergemann et al. (2005, chapters 3–4) and Fitzenberger et al. (2008) for details. While the IABS provides information on personal characteristics and employment histories, the combination of the transfer payment data and the training participation data is used to identify participation in different types of training programs.

3.2 Sample Selection

In this study, we analyze inflow samples into unemployment consisting of individuals living in West Germany who became unemployed after having been continuously employed for at least three months. The beginning of an unemployment spell is defined as the transition from regular employment (excluding subsidized employment and employment of very few hours with earnings below the lower social security threshold) to non–employment and subsequently being in contact with the employment office (not necessarily immediately), either through benefit receipt, program participation, or a job search spell.[16] This way, we focus on individuals closely attached to the labor market, which allows us to construct a control group that exhibits a similar employment history as the treated individuals. Furthermore, the beginning of unemployment defines a natural time scale to align treated and non–treated individuals. In order to exclude individuals in formal education or vocational training and individuals eligible for early retirement schemes, we only consider persons aged between 25 and 53 years at the beginning of their unemployment spell. Our evaluation focuses on participation in short–term training as the first training program that is attended in the course of an unemployment spell. Later participation in other active labor market programs is regarded as an outcome.

For the evaluation of ST00, we focus on an inflow sample into unemployment between the beginning of January 2000 and the end of June 2001. The analysis of ST8092 is based on an inflow sample into unemployment from January 1980 to January 1991.[17] We consider participation in short–term training within the first two years of an unemployment spell. Thus, we evaluate ST8092 programs starting from January 1980 until their abolition in December 1992 and ST00 programs starting between January 2000 and June 2003. For the earlier time period, the data allow us to follow all individuals until the end of 1997. Therefore, we are able to estimate long–term effects of the ST8092 programs for all participants in our data. We follow the individuals in the more recent sample until the end of 2004.

In the sample covering the early 2000s, we distinguish two types of short–term training programs: the first one puts more emphasis on training the job–seeker (QST00), while the second one focuses on monitoring and testing the availability for work (MST00). We argue that the QST00 variant of ST00 is more similar to the

[16] In the IEBS we can identify employment subsidized by the labor agency and thus exclude this from our definition of regular employment. This is unfortunately not possible for the 1980s and 1990s.

[17] This implies that the same individual may appear more than once in our evaluation sample. We take account of multiple inclusions of the same individual in the sample when calculating the standard errors.

ST8092 programs.

Figures 4 and 5 in the on–line appendix show the distribution of starting dates, for both ST8092 and ST00. It turns out that program starts occur earlier during the unemployment spell for ST00 compared to ST8092. The distribution for ST8092 is fairly flat during the first year and falls slowly over the second year. Note that a sizeable share of program starts occurs in the second year – and some even after two years. For our comparison of ST00 and ST8092, we choose the same time windows distinguishing between treatment starting during months 0 to 6 of the unemployment spell (stratum 1), treatment starting during months 7 to 12 (stratum 2), and treatment starting during months 13 to 24 (stratum 3). The number of participants and the size of the control group for each specification are depicted in table 3. Our choice of strata allows for a reasonable number of participants in each stratum for ST8092, MST00, and QST00 (the smallest involving 126 participants).

— Insert table 3 about here —

When choosing the number and width of the temporal strata there exists a trade–off between bias (by smoothing over starting dates within a stratum and not analyzing the dynamic assignment process within a stratum) and sample size. While there are generally many comparison observations, the number of treated observations is a constraining factor, especially for ST8092. Because of a low unconditional participation rate in ST8092, the participant sample sizes are rather small. Therefore, we pool over the years 1980–1992 in order to obtain sizeable treatment samples in the three unemployment strata. This approach results in a large overall group of non–participants considered in the analysis.[18] We apply kernel matching techniques for treatment effects estimation, which allows us – in contrast to nearest neighbor matching – to make use of the large control group sample sizes when we estimate the counterfactual outcomes (see the next section for further details). Small treatment group sizes translate into correspondingly larger standard errors and thus insignificant treatment effects. Put differently, only sufficiently strong positive or negative effects will prove significant and in such cases the results are informative. We prefer our approach over parametric methods that may lead to more precisely estimated effects, but may possibly be biased because of unjustified functional form assumptions.

We consider three outcome variables: monthly employment status, monthly participation rate in a longer–term training program later in the unemployment spell,

[18] In some cases, the control samples are so large, that we can only use a random subsample.

and earnings over six months. In addition, we also estimate the average treatment effect for employment over a longer time period after treatment start, a parameter which may be more precisely estimated than monthly effects. The propensity scores and the treatment effects are estimated separately for the different program types, strata, and for both men and women.

4 Evaluation Approach

Our goal is to analyze the effect of short–term training programs on three outcome variables, namely the individual monthly employment dummy, earnings within a half–year interval, and an individual monthly participation dummy in a longer–term training program.[19] The treatment we evaluate is participating in a short–term training program as a first training program over the course of an unemployment spell against the alternative of not participating in a short–term training program as a first training program. This alternative includes the case of participating in a long–term training program as first training program or no–participation in any training program.[20] We estimate the average treatment effect on the treated (ATT) of short–term training as first treatment against this alternative. Extending the static treatment approach to a dynamic setting, we follow Sianesi (2004) and apply the standard static treatment approach recursively depending on the elapsed unemployment duration. The implementation builds upon the approach developed in Fitzenberger and Speckesser (2007), Biewen et al. (2007), and Fitzenberger et al. (2008).

Our empirical analysis is based upon the potential–outcome–approach to causality, see Roy (1951), Rubin (1974), and the survey of Heckman et al. (1999). Let the two potential outcomes be $\{Y^0, Y^1\}$, where Y^1 represents the outcome associated with participation in a short–term training program and Y^0 is the outcome when the individual does not participate in a short–term training program. For each

[19]Individual participation is measured as a dummy variable equal to one when the individual participates in a longer–term training program in the respective month, irrespective of whether he or she is still in the same unemployment spell or has already experienced transitions in and out of employment.

[20]For the more recent data (ST00), the alternative also includes participation in other active labor market programs besides long–term training. But for the older data (ST8092), we cannot observe other programs besides training. We cannot distinguish between subsidized employment and regular employment in the older data. We conduct a sensitivity analysis to explore to what extent differences in the information considered affect our estimated treatment effects of ST00. The differences are negligible; see the discussion in section 5 and figures 9 and 10 in the on–line appendix.

individual, only one of the two potential outcomes is observed and the other outcome is counterfactual. We focus on the average treatment effect on the treated (ATT) of participating in a short–term training program against non–participation in a short–term training program at some given elapsed unemployment duration (i.e. treatment versus waiting).

Fredriksson and Johansson (2008) argue that a static evaluation approach, which assigns unemployed individuals to a treatment group and a non–treatment group based on the treatment information observed in the data within a fixed time window, yields biased treatment effects. This is because the definition of the control group conditions on future outcomes or future treatment. Sianesi (2004) argues that all unemployed individuals are potential future participants in active labor market programs, a view which is particularly plausible for countries with comprehensive systems of active labor market policies (like Sweden or Germany). In Germany, active labor market programs are implemented at a fairly large scale in international comparison. While unemployed, job–seekers are continuously at risk of being assigned to an active labor market program. This discussion implies that a purely static evaluation of the different training programs is not warranted. Following Sianesi (2004, 2008), we analyze the effects of the first participation in a short–term training program during the unemployment spell considered *conditional on the starting date of the treatment, relative to the start of the unemployment spell.*

We analyze treatment effects conditional on elapsed unemployment at program start. We evaluate the effect of participating in short–term training as the first treatment during the unemployment spell considered. The ATT parameter for treatment in month u of the unemployment spell is given by

$$\theta(u,\tau) = E(Y^1(u,\tau)|T_u = 1, U \geq u-1, T_1 = ... = T_{u-1} = 0) \tag{1}$$

$$-E(Y^0(\tilde{u},\tau)|T_u = 1, u \leq \tilde{u} \leq \bar{u}, U \geq u-1, T_1 = ... = T_{u-1} = 0) \ ,$$

where u denotes the month of unemployment when the treatment starts, U the completed duration of unemployment, and T_u is a dummy variable indicating the start of treatment. $\tau = 0, 1, 2, \ldots$ counts the elapsed time since the beginning of treatment. $Y^1(u,\tau)$ is the period τ potential outcome associated with treatment in period u. $Y^0(\tilde{u},\tau)$ denotes the potential outcome in the non–treatment case, where $\tilde{u}$ indicates non–participation between month u and $\bar{u}$ ($\bar{u}$ is the end of the stratum of elapsed unemployment considered).[21] The potential outcomes condition

[21] The requirement of non–participation until the end of the stratum does not exclude the possibility of exiting unemployment between period u and period $\bar{u}$. The control group comprises all

upon remaining unemployed at least until month u, i.e. $U \geq u-1$, and not receiving treatment before month u, i.e. $T_1 = ... = T_{u-1} = 0$. Non–participation during the current stratum involves the possibility of treatment in a later stratum, which implies that, after the end of the current stratum, $Y^0(\tilde{u}, \tau)$ includes potential outcomes for treatments at later points in time. Our treatment parameter (1) mirrors the decision problem of the caseworker and the unemployed who recurrently during the unemployment spell decide whether to start a short–term training program now or to postpone participation to the future. The treatment effect we actually estimate is the average within a stratum

$$\theta(\tau) = \sum_u g_u \theta(u, \tau) \ ,$$

where the average is taken with respect to the distribution g_u of starting dates u within the stratum.

We evaluate the effects of treatment assuming the following dynamic version of the conditional mean independence assumption (DCIA)

$$(2) \qquad E(Y^0(\tilde{u}, \tau)|T_u = 1, u \leq \tilde{u} \leq \bar{u}, U \geq u-1, T_1 = ... = T_{u-1} = 0, X)$$

$$= E(Y^0(\tilde{u}, \tau)|T_{\tilde{u}} = 0, u \leq \tilde{u} \leq \bar{u}, U \geq u-1, T_1 = ... = T_{u-1} = 0, X) \ ,$$

where X are observed characteristics that are time–invariant within an unemployment stratum. We effectively assume that conditional on X, and conditional on remaining unemployed and not receiving treatment at least until period $u-1$, individuals who receive treatment in period u, $T_u = 1$, are comparable in their non–participation outcome to individuals who do not receive treatment during the stratum considered, $T_{\tilde{u}} = 0, u \leq \tilde{u} \leq \bar{u}$.

In our study, we apply propensity score matching building on Rosenbaum and Rubin's (1983) result on the balancing property of the propensity score in the case of a binary treatment. To account for the dynamic treatment assignment, we estimate the probability of treatment given that unemployment lasts long enough to make an individual 'eligible'. For treatment starting during months 1 to 6 (stratum 1), we take the total inflow sample of unemployed, and estimate a probit model for treatment during stratum 1. The non–participation group includes those unemployed who either never participate in the treatment or who start a treatment after month

---

individuals whose unemployment spell lasts at least $u-1$ periods and not more than $\bar{u}$ periods and who do not participate in any program during the stratum considered. Some of them may exit to employment before the end of the stratum.

6. For treatment during strata 2 (months 7 to 12) and 3 (months 13 to 24), the basic sample consists of those unemployed who are still unemployed in the last month of the previous stratum. Implicitly, we assume that the actual beginning of treatment within a stratum is random conditional on X. Many unemployment spells in Germany are often quite long relative to other countries like the US. This makes our assumption not seem unreasonable in combination with the fact that available training programs often only start at fixed calendar dates. Note that we only use non–participants to estimate the treatment effect who are unemployed in the month before treatment start, see below.

We implement a stratified local linear matching approach by imposing that the matching partners for a treated individual are still unemployed in the month before treatment starts, i.e. we exactly align treated and nontreated individuals by elapsed unemployment duration in months. In addition, we exactly align treated and controls by the calendar month in which the unemployment spell began. The expected counterfactual outcome for non–participation is obtained by means of a local linear regression on the propensity score. We use a cross–validation procedure to obtain the bandwidth minimizing the squared prediction error for the average of the non–participation outcome for the nearest neighbors of the treated individuals (see Bergemann et al., 2009; Galdo et al., 2008).[22] An estimate for the variance of the estimated treatment effects is obtained through bootstrapping based on 250 resamples. We resample individuals. This way, we take account of the sampling variability in the estimated propensity score and we obtain standard errors which are clustered at the individual level.

As a balancing test (see Lee, 2012, for a recent critical assessment of balancing tests), we use the regression test suggested in Smith and Todd (2005) to investigate whether the covariates are balanced sufficiently by matching on the estimated propensity score.[23] For this purpose, each regressor in a given propensity score specification is regressed on a flexible polynomial of the predicted propensity score and interactions of this polynomial with the treatment dummy. We then determine the number of covariates in each specification for which the balancing test passes, i.e. the null

[22]The method used here is an extension of the cross–validation procedure suggested in Bergemann et al. (2009). Galdo et al. (2008) define and implement various weighted cross–validation approaches that consider the location of the treated units in the selection of the smoothing parameters. An extensive Monte Carlo study shows efficiency gains from all methods that take account of the location of the treated units. Galdo et al. also provide a recent summary of the literature.

[23]Lee (2012) performs a Monte Carlo study of different balancing tests. Permutation versions of various parametric tests, which are similar to a bootstrap, tend to perform best. The regression test of Smith and Todd (2005) seems to reject too often. In total, the size of this test is much larger than the nominal size of the single tests conducted for each of the regressors in the propensity score.

hypothesis that the polynomial of the propensity score interacted with the treatment dummy equals zero is not rejected. Furthermore, we investigate whether treated and matched nontreated individuals differ significantly in their outcomes before the beginning of the unemployment spell. We estimate these differences in the same way as the treatment effects after the beginning of the program. By construction, treated individuals and their matched counterparts exhibit the same unemployment duration until the beginning of treatment.

We also investigate the heterogeneity of the ATT with respect to calender time. For all treated individuals in the ST8092 sample, we calculate the cumulated individual treatment effects by summing the individual monthly effects over the months of training participation as well as the months after completion of training. We then run a linear regression of these cumulated individual effects on dummy variables for the different calendar years. This way we can assess whether program effects vary by the macroeconomic situation.

We conclude this section with an exposition of why identification based on the DCIA (2) is a credible strategy in our application. In our institutional context, the participation probability depends upon the variables that also determine re–employment prospects. Consequently, all individuals are considered as matching partners who left employment at the same calendar date as the treated individuals (i.e. we require that unemployment started in the same calendar month) and who have experienced the same elapsed unemployment duration up to the considered program starting date. Furthermore, we consider a rich set of individual characteristics and detailed information on previous employment experiences in the propensity score estimation. E.g., we consider information on schooling and vocational education, region of residence, occupation, industry as well as the remaining period of entitlement to unemployment benefits. We use detailed information on past employment and unemployment spells to proxy for 'soft factors' that may influence participation such as the ability or motivation of the unemployed.[24] Finally, as participation in training occurred at a fairly large scale in Germany, assignment was not very targeted and rather driven by the regional supply of programs. Moreover, caseworkers had little guidance on 'what works for whom'. Supporting our point of view, Schneider et al. (2006) suggest that, until the end of 2002, training assignment was strongly driven

[24] Caliendo et al. (2004) investigate the employment effects of job creation schemes in 1999/2000 in Germany based on an administrative data set similar to ours. They were able to combine the administrative data with survey data containing direct information on the motivation of participants that is not available in the administrative data. They find that controlling in addition for the motivational variables from the survey does not result in notable differences in the estimated program effects compared to using only the administrative data. This evidence also supports our point of view.

by the supply of available courses.

5 Empirical Results

5.1 Estimation of the Propensity Scores

We fit the propensity scores separately for each of the 18 groups defined by program type (ST8092, MST00, QST00), gender, and stratum (program start in months 0 to 6, 7 to 12, or 13 to 24 of the unemployment spell). In each case, we run an extensive specification search. The final specification was chosen based on consideration of which variables may (according to institutional and economic knowledge) drive the selection into programs, based on the statistical significance of the variables included, and based on the balancing tests described above.[25] The final specifications include 15 to 31 covariates. The Smith and Todd (2005) balancing test is passed in almost all cases at a 1% significance level, except for one specification where we reject the null hypothesis for one regressor when using the quartic of the propensity score (tables 6 and 9 in the additional on–line appendix). Regarding the 5% level we still pass 895 of 928 tests (both cubic and quartic regressions counted).[26]

A closer look at the estimation results for the propensity scores reveals that the following information is particularly relevant: region, age, schooling degree, occupational qualification, family status, children, foreign or German nationality, time spent in different labor market states during the last three years (before the unemployment spell), remaining claim on unemployment benefits, industry of last employment, last occupation, last wage, reason for the end of last employment, year or quarter the person became unemployed in, health status, past health problems, information on program dropout during the last three years, (benefit) sanctions during the last three years, participation in a program supporting rehabilitation and social integration, indication of lack of motivation during the last three years.

The variables occupation, reason for the end of last employment, health status, past health problems, penalties and benefit sanctions during the last three years, participation in a program with a social work component, indication of lack of motivation within the last three years, child younger than 10 years, and classification

[25] Detailed estimation results are available in tables 5 and 8 in the additional on–line appendix.

[26] Monte Carlo evidence in Lee (2012) suggests that the Smith and Todd (2005) balancing test rejects too often, see footnote 23. Therefore, we think that the covariates of treated and matched controls are well matched.

of districts according to local labor market conditions are not available for the older data. We suspect that we capture the information implicitly by using detailed variables on the individual's labor market history. As a check, we also conduct a sensitivity analysis for the estimation of employment effects by dropping from the more recent data those variables not available in the older data. We summarize the results of this test in the next section.

In the following, we discuss those variables which are less standard: by using variables which count the days spent in different labor market states (employment, receipt of unemployment benefit or unemployment assistance, participation in training or just any contact with the labor agency) during the last three years before the start of the unemployment period in focus (or alternatively dummies if a particular labor market status occurred within the last three years), we account for the fact that some job–seekers experienced more or less time in unemployment before, while others have been continuously employed during the last three years. The reason for the end of the last employment is captured using dummy variables indicating whether it was a displacement, the end of a fixed–term contract or a voluntary quit. This variable may capture aspects of the individual's employment history beyond counting days in employment. For example, if the job–seeker held a fixed–term contract before, he may have been more likely to already look for a new job while in employment. The remaining claim on unemployment benefits is calculated by ourselves accounting for changing rules and measured in days. The remaining time a job–seeker may claim benefits is supposed to influence his or her job search behavior and may also influence the caseworker's decision to assign a program. If a person did not complete a scheduled program within the last three years before the start of the current unemployment period, the caseworker may be more likely to use an ST00 program to test his or her willingness to cooperate. This is also the reason for including a dummy if the individual had been given a sanction before. In addition to being potentially important for the selection into programs, these pieces of information are likely to be relevant for the individual's job search behavior. The dummies indicating whether the job–seeker has participated in a program with a social work component in the past and whether there is any indication in the data for lack of motivation (like repeated absence on the day of a scheduled interview) reflect potential problems. These variables may be relevant for the decision of the caseworker to assign an ST00 program.

5.2 Estimated Treatment Effects

The evaluation results for the employment effect of participating in short–term training as the first training program vs. waiting are shown in figures 1 to 3. Each graph displays the average treatment effect on the treated (ATT), i.e. the difference between the actual and the counterfactual employment outcome averaged over those individuals who participate in the program under consideration. More precisely, we compare the actual employment outcome of the treated to the employment outcome these individuals would have had, *had they not taken part in short–term training as a first training program in the respective time window of their unemployment spell.* We distinguish between programs starting in three different time windows (strata) of elapsed unemployment: 0 to 6 months (stratum 1), 7 to 12 months (stratum 2), and 13 to 24 months (stratum 3). We evaluate treatment effects at different points in time. On the time axis in our graphs, positive values denote months since the program start, while negative values represent pre–unemployment months. We omit the period between the start of unemployment and the start of the program where both the control and treatment groups are unemployed. The dashed lines around the estimated ATT are bootstrapped 95 percent pointwise confidence bands. Treatment effects for a particular month are statistically significant if zero is not contained in the confidence band.

— Insert figure 1 about here —

Figure 1 shows the estimated treatment effects for the short–term training programs in the early 2000s with a strong focus on qualification (QST00). The results for men are given in the left column, while those for women are shown in the right column. During the program and in the period immediately following the end of the program, participants typically have a lower monthly employment probability than they would have had if they had not participated in the program. This is the so–called lock–in effect. Figure 1 suggests relatively short (1 to 4 months) and not very pronounced lock–in effects. These lock–in effects are a bit deeper for stratum 1 (about 7 percentage points) than for the later strata (2 to 4 percentage points). After the short lock–in period, the difference between actual and counterfactual employment outcomes of participants turns positive. We find significantly positive effects for men in the second and third strata (i.e. those men who have been unemployed for at least half a year before entering a program) and for women in the first and second strata, but not in the third stratum. However, the point estimates for the latter are positive after six months. The largest employment effects occur between month 12 and month 18. In the four groups with significant effects, the size of the effects

reaches 9 to 17 percentage points. After 18 months, the effects tend to decline a little, but positive ATTs of 7 to 12 percentage points persist until the end of the observation period (18 to 36 months after program start depending on the stratum).

These long lasting effects are quite remarkable given that the programs last only a few weeks. First, as we do not exclude participants who attend a second training program after short–term training, but regard the second program as an outcome, it could well be that the long–term effects are to some degree due to long–term training programs which have been started as a result of the short–term training program. This would imply that short–term training serves as a bridge into more intensive training programs and this combination eventually leads to positive employment effects. Second, if a high share of those who do not participate in the stratum considered take long–term training later, this may also lead to positive treatment effects for the participants for the time the (former) non-participants are locked into programs. But as our results on future participation suggest that participating in long–term training is more frequent for participants in short–term training (see the discussion of figures 7 to 9 below), and as lock–in effects of short–term training are short the former argument seems to dominate.

— Insert figure 2 about here —

Figure 2 presents the corresponding results for the short–term training programs in the early 2000s we classified as having a strong focus on testing the availability and willingness to work as well as the skills of job–seekers (MST00). The graphs suggest that, while the point estimates of the monthly average treatment effects are mostly positive, they generally fail to be clearly significant. The only exception are women who receive treatment in months 7 to 12 (stratum 2) of their unemployment spell. After a small and very short lock–in effect, we first observe a small and insignificant positive effect. Rising steadily over more than two years, it turns significant after 9 months and eventually reaches 16 percentage points. This picture fits into the scenario that part of the participants attend a second program as a result of the MST00 program and this combination of programs eventually may lead to positive employment effects. The trend of the treatment effect is similar for men and women in stratum 1, but in these cases the level is much lower and the effect is insignificant. The lock–in effects follow a similar pattern for MST00 as for QST00.

In sum, participants of MST00 seem to have benefited less from their program than participants of QST00 from the program they were assigned to. This does not necessarily imply that participants in MST00 would benefit from participating

in QST00.[27] We have no information if the program costs per day differ between MST00 and QST00, but as both types of ST00 usually consist of teaching a medium size group in classrooms, per day costs are likely to be similar. The average direct cost for a month of ST00 was about €550 in the years 2000 to 2004 (Osikominu, 2009). But the median length of QST00 is 40 days in our sample, while the median length of MST00 is only 19 days. So if per day costs were the same between these two program types and equally distributed over the length, the median length MST00 program would cost €343 and the median length QST00 program would cost €721. Thus, QST00 produces higher gains but is also more than twice as expensive as MST00.

— Insert figure 3 about here —

Results for short–term programs in the period 1980–1992 (ST8092) are given in figure 3. The estimated monthly average treatment effects are positive after an initial lock–in effect. Remarkably, the lock–in period is typically longer for ST8092 than for ST00. Also, the monthly ATTs of ST8092 are mostly smaller than those of QST00 and statistically insignificant. Only for women unemployed for more than one year (stratum 3) do the results show significantly positive treatment effects of 7 to 10 percentage points between month 6 and month 20 after program start. For the other groups the effects are – although for the time after the lock–in period always positive – insignificant. Interestingly, for most groups the employment effect increases between month 18 and month 26 after treatment start. As discussed before, this is likely due to participation in another training program as a result of participation in short–term training. In sum, ST8092 programs seem to be less successful in bringing people back to employment compared to ST00, in particular QST00. However, ST8092 were explicitly targeted to hard–to–place individuals.

One may wonder whether we miss an important part of the selection effects in the older data because we only have a reduced set of variables available for matching compared to the data available for ST00. As a sensitivity check for the employment effects, we redid the analysis for QST00 and MST00, this time not considering in the more recent data the information not available in the older data.[28] We re–estimated the propensity scores based on a restricted set of covariates that is also available in the older data. In addition, we redefined the outcome dummy

[27] If treatment effects differ by the characteristics of the unemployed, a proper comparison of the effect of one program against another would require balancing the characteristics in the two treatment groups, see e.g. in Fitzenberger et al. (2008) or Biewen et al. (2007). Such a pairwise analysis would have been beyond the scope of this paper.

[28] We are grateful to Jeff Smith for this suggestion.

for employment analogously to the older data set. In particular, participants in subsidized employment schemes are now counted as regularly employed. We use the same bandwidths as for the benchmark estimates reported in figures 1 to 3. The differences in the estimated treatment effects are negligible, see figures 9 to 10 in the on–line appendix. This supports our presumption that the variables used for the older data are sufficient for controlling for the selectivity of participants.

— Insert tables 4 and 5 about here —

Table 4 shows averages of the monthly ATTs from month six after program start until the end of the observation period as a way of condensing employment effects after the end of the lock–in period. In four cases for QST00 and two cases for MST00, the figures reported in table 4 suggest highly significant employment effects between 6 and 14 percentage points, for the other groups the effects are smaller and not (or only slightly) significant. The results for the ST8092 programs suggest significantly positive ATTs for women in strata 2 and 3 and for men in stratum 2 in the range 6.2 to 7.4 percentage points despite the mostly insignificant point estimates in figure 3. The effects for the three other cases are smaller in size and not significant. For comparison, table 5 shows the averages of the ATTs including the lock–in effects: these are also always positive and only one case which is strongly significant without the lock–in effect changes to being only marginally significant when the lock–in period is included. This suggests that the lock–in effects do not dominate the overall program effects.

— Insert table 6 about here —

Table 6 shows gains and losses in terms of months employed cumulated over up to two years (four years for the ST8092 programs, respectively) after program start as a way of condensing the graphical results in figures 1 to 3. This measures by how much participation in short–term training increases the time spent in employment in a given time period, when initial negative and subsequent positive employment effects are weighed against each other. While the gains are very small or not even positive over the first 6 months, they increase for most groups over a longer period. The positive employment effects of QST00 are confirmed again. The effects of QST00 cumulated over 24 months are in general larger than those of MST00 and ST8092. In the cases where we find significant effects after 24 months, these lie in a range between one and 2.5 months. For example, men and women participating in QST00 after having been unemployed between 7 and 12 months (stratum 2) gain 2.4

months in employment during the first 24 months after the program start. Women participating in MST00 in stratum 2 gain 2.6 months in employment in two years. For the ST8092 programs, there are surprisingly high gains for women who were long–term unemployed before the program. After 48 months, we find significantly positive cumulated employment effects of ST8092 programs (in one case significance is only at the 10% level) for women in strata 2 and 3 and for men in stratum 2.

— Insert figures 4, 5, and 6 about here —

The estimated treatment effects on earnings are shown in figures 4 to 6. The ATTs shown are the difference in earnings in a half–year interval (in 1995 €) between participants and matched controls. The pattern of the effect sizes and significance is similar to the one found for employment effects. In those cases where we find a significant earnings effect it is usually about €1000 per half–year. The largest effect is found for females participating in QST00 in the first stratum: it is highly significant, reaches €1000 in the second half–year after the start of treatment and is still about the same size three years after program start. The employment effect for this group is also clearly positive and relatively constant at about 7 percentage points until the end of the observation period. Given these numbers, it is possible that the earnings effect may be driven to a large degree or even completely by the employment gain.[29] In sum, the estimated effects on earnings reflect well the remarkably high employment effects for some groups.

— Insert figures 7, 8, and 9 about here —

Figures 7 to 9 show the estimated treatment effects on the monthly participation rates. Here, instead of the employment effect, we estimate the average effect of the short–term training program on the probability of participating in a longer–term further training program. Participating in ST8092 has a significant positive effect on the probability of participating in a long–term further training program in the months following the start of the ST8092 program for all groups (figure 9). The effect reaches its maximum (8 to 15 percentage points) within the first six months after the start of the ST8092 program and then slowly fades away. Two years after the start of ST8092 there remains a positive but small and not significant effect (except for men in stratum 1 for whom the effect is still slightly significant). Figure 7 also suggests positive treatment effects of participation in QST00 on future participation

[29] For example in 2001, gross monthly earnings were on average about €2500 (in 1995 €) for full time employed individuals in West Germany (Institut der deutschen Wirtschaft, 2008).

in long–term further training, but the effect is clearly significant only for the first stratum. The effects are smaller for QST00 as compared to ST8092: participation in QST00 increases the probability of participating in long–term training in a future month by a maximum of 3 to 7 percentage points. 18 months after the start of the QST00 program, the effect is still non–negligible for most groups. For participation in MST00 there is a positive and significant effect on future participation in long–term training for all groups. The effect turns positive after a shorter period for MST00 than for QST00, as MST00 programs are usually shorter. At its maximum the effect lies between 11 to 15 percentage points, and 18 months after the start of the treatment it has not declined by much.

— Insert table 7 about here —

Table 7 condenses these results by cumulating additional months in long–term training due to ST participation. For MST00 and ST8092 the effects are always highly significant, whereas only some of the effects are significant for QST00. Participation in QST00 leads to an estimated increase in long–term training of between 0.3 months (not significant) for women participating in stratum 2 and 1 month for men in stratum 1 within two years after the start. For MST00 the effects are higher with a maximal size of 2.6 months for men and women in stratum 1. The effects are a bit smaller for ST8092 than for MST00 with a maximum of 1.9 months for women in stratum 3. For ST00 the effects tend to be highest for those individuals participating soon after becoming unemployed (stratum 1), while in the earlier period for men the highest effect is found in stratum 2 and for women in stratum 3.

In sum, our results suggest that participation in ST00 typically leads to an increase in participation in long–term training of about one to two months. The effect is smaller for QST00 which is not surprising as these programs aim at a limited skill upgrade to directly enhance placement and less at an assessment. As a result, future program participation is a bit less of an issue. MST00 on the contrary has a strong focus on the assessment of the skills and opportunities of the job–seeker; thus, this program leads to a strong increase in future participation in long–term training, because one outcome of the assessment may be that a more substantive skill enhancement is necessary. For ST8092, guiding hard–to–place job–seekers into a long–term training program was an official goal. The estimated effects on participation rates reflect this goal. The typical duration of a long–term training program in Germany is very heterogeneous; durations of 6 to 24 months are very common. Thus, it is not surprising that the positive effect of ST participation on future program participation is still non–negligible two years after the start of the ST program. This reflects those

participants who start a very long program as a result of ST participation or who do not start the long–term program immediately after the end of the ST program.[30]

This study investigates program effects of ST8092 programs over 13 calendar years. Given this very long period, one could suspect that the employment effects differ over calendar time. Possibly, the activation effect of such programs is higher for the difficult–to–place when unemployment is low or the programs give the unemployed an additional edge when unemployment is high. In order to investigate effect heterogeneity, we regress the average individual treatment effects after the lock–in period (summarized in table 4) on year dummies, and the individual elapsed unemployment duration to investigate whether the effects differ between years. Bootstrap standard errors are calculated based on the resamples which are also used to calculate the standard errors of the treatment effect estimates. In addition, we do the same exercise for the treatment effects including the lock–in period and for the treatment effects during the lock–in period.

— Insert table 8 about here —

According to the results of these regressions, the ATTs do not differ over time, irrespective whether the lock–in period is included, not included, or taken alone: a χ^2–test of equality of the year dummies does not suggest any effect heterogeneity (table 8).[31] Thus, there is no evidence for the business cycle affecting the employment effects of short–term training, a finding which is in contrast to the results for long–term training programs in Lechner and Wunsch (2009). The result in Lechner and Wunsch (2009) is mainly driven by the lock–in effect which is typically very long and pronounced for long–term training (see for example Biewen et al. (2007)). The lock–in effect of ST8092 is, on the contrary, quite short, which may explain why our findings are different from Lechner and Wunsch (2009).

6 Conclusions

During the last decade, there was a greater emphasis on job search assistance, monitoring and testing work availability, as well as limited training to activate the unemployed (OECD, 2007). In Germany, the focus on activation strategies is reflected in

[30] Figures 6 to 8 in the on–line appendix provide further information on the incidence and duration of future program participation for participants and matched non–participants.

[31] The coefficients of these regressions are given in tables 10 to 13 in the additional on–line appendix. The coefficients are quite large, but very few of them are significant and there is no interpretable time pattern.

the shift in the early 2000s away from traditional longer further training programs lasting a couple of months up to two years to short–term training programs (ST00) with a median duration of four weeks. In fact, ST00 have become the largest program in Germany regarding the number of participants with 1.07 Million individuals entering such a program in 2007 (Bundesagentur für Arbeit, 2007, pp. 54, 57). Between 1980 and 1992, a similar large scale short–term training program was in place in Germany. These were the 'Programs According to Article 41a of the Employment Promotion Act' (ST8092).

This paper estimates the effects of short–term training programs in West Germany for the time periods 1980 to 1992 and 2000 to 2003 on the outcome variables employment, earnings and future participation in long–term training programs. This is the first paper to analyze these programs for the earlier time period and to estimate long–run effects on outcomes. Our results show that short–term training has mostly persistently positive and often significant employment effects. The effects are particularly strong for participants starting training during months 7 to 12 of the unemployment spell. We have to stress at this point that, because of the changing populations of unemployed across strata, our results do not imply that moving participation from another time period to months 7 to 12 would on average increase the employment gain from the treatment. We tend to find smaller effects for short–term training starting during the second year of the unemployment spell. When short–term training focuses on testing and monitoring search effort, there are smaller effects compared to when the focus is on training only. The lock–in periods lasted longer in the 1980s and 1990s compared to the early 2000s. The earnings effects basically reflect the pattern of the employment effects. Short–term training results in higher future participation in long–term training programs and this effect is much stronger for the earlier time period and for the testing and monitoring variant than for the qualification variant. The employment effects of the ST8092 programs did not change significantly by year between 1980 and 1992, i.e. there is no evidence for business cycle effects in contrast to the results for long–term training programs in Lechner and Wunsch (2009).

Our findings most likely reflect a change in active labor market policy between 1992 and 2000. In the 2000s, there is a strong focus on activating the unemployed. In contrast, in the 1980s and 1990s it was accepted policy to 'give the unemployed some time' and to encourage them to participate in long–term training programs when this seemed advisable and the unemployed were hard to place. Our results suggest that the policy reorientation towards activation did not result in worse employment outcomes. If anything, as far as comparable, ST00 programs with a focus

on training show better employment effects. As a caveat, we have to acknowledge, however, that the estimated treatment effects for the two time periods are obtained for different selective treatment samples, i.e. the effects can not be compared without accounting for these differences. ST8092 programs were particularly targeted at individuals with lower re–employment chances, while the target group for ST00 is broader. Any unemployed individual, for whom an assessment of his or her opportunities, a check of his or her willingness to work, or an upgrade of limited skills is considered necessary, may be assigned to ST00. However, a simple comparison of descriptive statistics of the participants in the earlier period and in the later period does not reflect these intended differences in assignment policies. On the one hand, participants in ST8092 tend to be a bit younger, are more likely to hold a vocational training degree and are more likely to have been employed two years prior to the start of the unemployment period in focus. On the other hand, there are fewer foreign nationals among them and fewer participants hold a university degree (tables 1 to 3 in additional on–line appendix).[32] The participants of QST00 tend to have slightly more favorable characteristics than the MST00 participants. Thus, there is no strong indication that different selective treatment samples push the treatment effect in a certain direction.

The fact that we find some long lasting effects of short–term training may be surprising given their short duration. These programs by themselves do not provide a sizeable human capital investment. Future research should investigate the hypothesis that the positive program effects can be traced back to the higher participation rates in long–term training programs. However, as one piece of evidence against this hypothesis, we find that in the case of QST00, which leads to particularly positive employment effects, effects on future participation are lowest. A thorough investigation of the hypothesis would require an evaluation approach for multiple sequential treatments as e.g. the one developed by Lechner and Miquel (2010), but it remains an open question whether the stringent identifying assumptions required are satisfied in applications like ours.

An overall assessment of the microeconomic effects of short–term training is not possible, because the necessary information for a comprehensive cost–benefit analysis is lacking in our data. But it is clear that ST00 programs are very inexpensive in comparison. The direct costs are estimated at €343 for a median length MST00 program and €721 for a median length QST00 program (see section 5.2). In addition, because of their short duration, ST programs involve short lock–in effects, so their

[32]This simple comparison is based only on a few characteristics and it neglects that these characteristics may differ in the population as well as in the sample of job–seekers between those periods. Thus, it may still be the case that assignment was different in the two periods.

cumulated employment effects may quickly become positive. In particular, QST00 participants enroling in months 7 to 12 of unemployment – for whom our results suggest a 2.4 months employment gain within two years and only a 0.3 to 0.7 month increase in future participation in long–term training (see tables 6 and 7) – are very likely to exhibit a positive net benefit. MST00 and ST8092 lead to slightly lower employment effects and at the same time to higher future participation in long–term training. In order to determine whether their net benefit is positive, it would be important to know the net benefit of long–term training, because the net benefit of MST00 and ST8092 participation will be driven largely by the net benefit of the much more expensive long–term training programs. Biewen et al. (2007), among others, find positive employment effects of long–term training for some groups of participants, but a cost–benefit analysis is not available. Even if the net benefit for the average participant of long–term training was known, this may not be a good measure for those who are assigned to long–term training after short–term training. On the one hand, it may well be that those participants who start long–term training after short–term training experience a higher benefit than the average participant because the monitoring in the short–term training program may lead to targeting the long–term program very well. On the other hand, it is possible that the initial participation in short–term training works as a starting point of locking a job–seeker into long–term programs, even if there is no need for further training. This would result in a lower than average benefit for the former short–term training participant. To estimate the incremental effect of long–term training following short–term training is beyond the scope of this paper.

As a further caveat, the estimated positive treatment effects do not necessarily reflect positive general equilibrium effects. Training programs may lead to substitution effects. A positive treatment effect for participants may reflect an advantage of participants relative to non–participants in applying for open positions and a negative employment effect for those non–participants who would otherwise have received a job offer. This may be relevant for short–term training because these programs do not aim at a sizeable human capital investment and will thus not lead to a considerable human capital enhancement in the work force (an increase in human capital may increase the number of vacancies). Such positive general equilibrium effects through human capital investment are less likely for short–term training. But activating a large number of job–seekers might also have a positive general equilibrium effect through vacancy creation, as the job search efforts and the motivation to find a job may increase. In contrast to substitution effects, deadweight losses seem less relevant for short–term training (in particular for MST00), first because these programs are often assigned to those job–seekers with a need for activation

and second because the direct program costs are low. In fact, the costs for all types of short–term training are fairly low, thus the repercussions through financing these programs are likely to be negligible. Unfortunately, our analysis does not allow us to sign the possible bias in our partial equilibrium estimates. For the early time period (ST8092), we think the bias is negligible because of the rather small size of the program.

In both periods, direct placement of job–seekers was an important goal of short–term training and the positive employment effects we find for both periods suggest that short–term training programs may be one useful component of active labor market policies to achieve this goal. ST8092 programs also had the goal to (and in fact did) increase future participation in long term training. MST00 also has a strong positive effect on future participation, but in the early 2000s this was not a goal itself, but rather one possible result of an assessment. The main focus in the period was on activation, implying that a cheap and short assessment program should directly lead to placement for a large fraction of participants. Our results suggest that using MST00 for this strategy had only limited success – QST00, focusing less on monitoring and more on teaching limited skills seems to be more successful in direct placement. But one has to take into account that MST00 is also shorter and cheaper than QST00 and that our evaluation strategy does not allow for direct comparison of these programs as participants may differ. So the policy implication of our results is that short and inexpensive training programs have been a useful component of active labor market programs in both periods and that it might be advantageous to use short–term training programs which aim at teaching some limited skills instead of very short programs which basically only aim at assessment and monitoring.

References

Abbring, J. and G.J. van den Berg (2003). "The Nonparametric Identification of Treatment Effects in Duration Models." *Econometrica* 71, 1491–1517.

Bender, S., A. Haas, and C. Klose (2000). "IAB employment subsample 1975–1995." *Schmollers Jahrbuch (Journal of Applied Social Science Studies)* 120, 649–662.

Bender, S., M. Biewen, B. Fitzenberger, M. Lechner, R. Miquel, A. Osikominu, M. Waller, C. Wunsch (2005). "Die Beschäftigungswirkung der FbW–Maßnahmen 2000 – 2002 auf individueller Ebene: Eine Evaluation auf Basis der prozesspro-

duzierten Daten des IAB – Zwischenbericht Oktober 2005." Goethe University Frankfurt and SIAW St. Gallen.

Bender, S., A. Bergemann, B. Fitzenberger, M. Lechner, R. Miquel, S. Speckesser, C. Wunsch (2005). "Über die Wirksamkeit von Fortbildungs– und Umschulungsmaßnahmen." *Beiträge zur Arbeitsmarkt– und Berufsforschung* 289, IAB, Nürnberg.

Bergemann, A. B. Fitzenberger, and S. Speckesser (2009). "Evaluating the Dynamic Employment Effects of Training Programs in East Germany Using Conditional Difference–in–Differences." *Journal of Applied Econometrics* 24, 797–23.

Biewen, M., B. Fitzenberger, A. Osikominu, and M. Waller (2007). "Which Program for Whom? Evidence on the Comparative Effectiveness of Public Sponsored Training Programs in Germany." IZA Discussion Paper, No. 2885, Bonn.

Bundesanstalt für Arbeit (1980–1993). "Förderung der beruflichen Bildung/Weiterbildung." Nürnberg (various issues).

Bundesanstalt für Arbeit (1985, 1994). "Amtliche Nachrichten der Bundesanstalt für Arbeit." Nürnberg (various issues).

Bundesagentur für Arbeit (2000–2005). "Arbeitsmarkt." Nürnberg (various issues).

Bundesagentur für Arbeit (2007). "Der Arbeits- und Ausbildungsmarkt in Deutschland – Dezember und das Jahr 2007." Monthly Report, Nürnberg.

Büttner, T. (2008). "Ankündigungseffekt oder Maßnahmewirkung? Eine Evaluation von Trainingsmaßnahmen zur Überprüfung der Verfügbarkeit." *Zeitschrift für Arbeitsmarktforschung* 41, 25–40.

Caliendo, M., R. Hujer, S.L. Thomsen, and C. Zeiss (2004). "Einfluss von motivationalen Merkmalen aus Befragungsdaten auf die Schätzung des Beschäftigungseffektes von ABM-Maßnahmen." Unpublished Manuscript, Goethe University Frankfurt.

Dobischat, R. and H. Wassmann (1981). "Die Teilnahme der Arbeitslosen an Maßnahmen zur Verbesserung der Vermittlungsaussichten und ihre Nutzung für die berufliche Weiterbildung." *Berufsbildung in Wissenschaft und Praxis* 6/81, 17–19.

Dyke, A., C. Heinrich, P. Mueser, and K. Troske (2006). "The Effects of Welfare–to–Work Program Activities on Labor Market Outcomes." *Journal of Labor Economics* 24, 567–607.

Fay, R. (1996). "Enhancing the Effectiveness of Active Labour Market Policies: Evidence from Programme Evaluations in OECD countries." Labour Market and Social Policy Occasional Papers, 18, OECD, Paris.

Fitzenberger, B., A. Osikominu, and R. Völter (2008). "Get Training or Wait? Long–Run Employment Effects of Training Programs for the Unemployed in West Germany." *Annales d'Economie et de Statistique* 91–92, 321–355.

Fitzenberger, B. and S. Speckesser (2007). "Employment Effects of the Provision of Specific Professional Skills and Techniques in Germany." *Empirical Economics* 32, 529–573.

Fredriksson, P. and P. Johansson (2008). "Dynamic Treatment Assignment – The Consequences for Evaluations using Observational Data." *Journal of Business and Economic and Statistics* 26, 435–445.

Galdo, J. Smith, J. and D. Black (2008). "Bandwidth Selection and the Estimation of Treatment Effects with Unbalanced Data." *Annales d'Economie et Statistique* 91–92, 189–216.

Heckman, J. R.J. LaLonde, and J.A. Smith (1999). "The Economics and Econometrics of Active Labor Market Programs." In: O. Ashenfelter and D. Card (eds.), *Handbook of Labor Economics*, Vol. 3 A, Amsterdam: Elsevier Science, 1865–2097.

Heinrich, C., P. Mueser, K. Troske, K. Jeon and D. Kahevioglu (2009). "New Estimates of Public Employment and Training Program Net Impact: A Nonexperimental Evaluation of the Workforce Investment Act Program." IZA Discussion Paper No. 4569.

Hujer, R., S.L. Thomsen, and C. Zeiss (2006). "The Effects of Short–Term Training Measures on the Individual Unemployment Duration in West Germany." Discussion Paper 06–065, ZEW Mannheim.

Hummel, E., P. Jacobebbinghaus, A. Kohlmann, M. Oertel, C. Wübbeke, and M. Ziegerer (2005). "Stichprobe der Integrierten Erwerbsbiographien, IEBS 1.0." FDZ Datenreport 6/2005, Nürnberg.

Institut der deutschen Wirtschaft (2008). *Deutschland in Zahlen 2008.* Köln: Deutscher Instituts–Verlag.

Jacobebbinghaus, P. and S. Seth (2007). "The German Integrated Employment Biographies Sample IEBS." *Journal of Applied Social Science Studies (Schmollers Jahrbuch)* 127, 335–342.

Kluve, J. (2010). "The Effectiveness of European Active Labor Market Policy." *Labour Economics* 17(6), 904–918.

Kurtz, B. (2003) "Trainingsmaßnahmen – Was verbirgt sich dahinter?" IAB Werkstattbericht No. 8, Nürnberg.

Lechner, M. and R. Miquel (2010). "Identification of the effects of dynamic treatments by sequential conditional independence assumptions." *Empirical Economics* 39, 111–137.

Lechner, M. and C. Wunsch (2009). "Are training programs more effective when unemployment is high?" *Journal of Labor Economics* 27, 653–692.

Lechner, M., R. Miquel, and C. Wunsch (2011). "Long–Run Effects of Public Sector Sponsored Training in West Germany." *Journal of the European Economic Association* 9(4), 742–784.

Lee, W.–S. (2012). "Propensity Score Matching and Variations on the Balancing Test." *Empirical Economics*, forthcoming.

Martin, J.P. and D. Grubb (2001). "What works and for whom: A review of OECD countries' experiences with active labour market policies." *Swedish Economic Policy Review* 8, 9–56.

Osikominu, A. (2009). "Quick Job Entry or Long–Term Human Capital Development? The Dynamic Effects of Alternative Training Schemes." IZA Discussion Paper No. 4638.

OECD (2007). "Activating the Unemployed: What Do Countries Do?" Employment Outlook, Chapter 5, Paris.

Plaßmann, G. (2002). "Der Einfluss der Arbeitslosenversicherung auf die Arbeitslosigkeit in Deutschland." *Beiträge zur Arbeitsmarkt– und Berufsforschung, BeitrAB* 255.

Rosenbaum, P.R. and D.B. Rubin (1983). "The Central Role of the Propensity Score in Observational Studies for Causal Effects." *Biometrika* 70, 41–55.

Roy, A.D. (1951). "Some Thoughts on the Distribution of Earnings." *Oxford Economic Papers* 3, 135–146.

Rubin, D.B. (1974). "Estimating Causal Effects of Treatments in Randomized and Nonrandomized Studies." *Journal of Educational Psychology* 66, 688–701.

Schneider, H. (1981). "Erfahrungen mit "41a"." *Arbeit und Beruf* 4/1981, 97–99.

Schneider, H., K. Brenke, D. Hess, L. Kaiser, J. Steinwede, and A. Uhlendorff (2006). "Evaluation der Maßnahmen zur Umsetzung der Vorschläge der Hartz–Kommission – Modul 1b: Förderung beruflicher Weiterbildung und Transferleistungen." IZA Research Report, No. 7.

Sianesi, B. (2004). "An Evaluation of the Swedish System of Active Labor Market Programs in the 1990s." *Review of Economics and Statistics* 86, 133–155.

Sianesi, B. (2008). "Differential Effects of Active Labour Market Programs for the Unemployed." *Labour Economics* 15, 379–499.

Smith, J.A. and P. Todd (2005). "Rejoinder." *Journal of Econometrics* 125, 365–375.

Stephan, G. S. Rässler, and T. Schewe (2006). "Das TrEffeR–Projekt der Bundesagentur für Arbeit – die Wirkung von Maßnahmen aktiver Arbeitsmarktpolitik." *Zeitschrift für ArbeitsmarktForschung* 39, 447–465.

Wunsch, C. and L. Lechner (2008). "What Did All the Money Do? On the General Ineffectiveness of Recent West German Labour Market Programmes." *Kyklos* 61, 134–174.

Tables

Table 1: Entries into Active Labor Market Programs in West Germany 1979–1992 (in Thousand)

	1979	1980	1981	1982	1983
Training Programs	209	247	280	266	306
– Further Training	149	162	190	189	220
– Short–Term Training	0.7	14	25	23	24
– Retraining	31	38	47	42	42
– On–the–Job Training	29	33	17	11	20
Job Creation Schemes	20	15	14	8	23
	1984	1985	1986	1987	1988
Training Programs	353	409	530	596	566
– Further Training	260	298	374	420	420
– Short–Term Training	30	38	52	63	29
– Retraining	43	45	59	65	66
– On–the–Job Training	19	28	45	49	51
Job creation schemes	26	34	41	42	38
	1989	1990	1991	1992	
Training programs	490	574	594	575	
– Further Training	361	383	421	418	
– Short–Term Training	27	59	53	47	
– Retraining	61	63	70	81	
– On–the–Job Training	41	68	49	29	
Job Creation Schemes	28	27	28	18	

Source: Bundesanstalt für Arbeit (1980–1993), Bundesanstalt für Arbeit (1985, 1994), own calculations.

Table 2: Entries into Active Labor Market Programs in Germany and West Germany 1999–2004 (in Thousand)

	1999	2000	2001	2002	2003	2004
	Germany					
Qualification Programs	1,108	1,154	1,069	1,457	1,502	1,435
– Further and Retraining	491	552	450	456	255	185
– Short–Term Training	432	477	565	877	1,064	1,188
Employment Subsidies	538	459	465	538	808	909
Placement and Advisory Services	532	601	742	934	1,460	2,795
Job Creation Schemes	353	314	246	217	189	166
Specific Measures for Youths	530	446	496	447	389	408
Other	312	391	478	469	217	105
Total	3,373	3,365	3,496	4,062	4,565	5,818
	West Germany					
Qualification Programs	714	770	643	972	985	958
– Further and Retraining	307	338	261	273	161	124
– Short–Term Training	265	286	339	545	690	789
Employment Subsidies	245	225	206	245	365	451
Placement and Advisory Services	286	279	296	375	640	1,447
Job Creation Schemes	96	89	73	63	39	42
Specific Measures for Youths	426	364	191	210	262	270
Other	231	125	370	289	17	85
Total	1,997	1,852	1,778	2,154	2,308	3,253

Source: Bundesagentur für Arbeit (2001, 2002, 2003, 2004, 2005), own calculations.

Table 3: Participation in Short–Term Training as a First Training Program for the Inflow Samples into Unemployment

Stratum	Months 1 to 6	Months 7 to 12	Months 13 to 24
Short–Term Training Between 1980 and 1992 (ST8092)			
Male Participants	165	201	183
Male Comparisons	59921[a]	25674[a]	15631
Female Participants	145	145	167
Female Comparisons	35782[a]	22970	17020
Short–Term Training in the Early 2000s (ST00)			
Male Participants QST00	559	221	211
Male Participants MST00	531	177	214
Male Comparisons	20979	8337	5122
Female Participants QST00	537	214	130
Female Participants MST00	325	126	115
Female Comparisons	13848	7070	4975

Notes: QST00 refers to the qualification variant of short–term training administered in the early 2000s and MST00 to the monitoring variant. For entries marked with a [a], we randomly selected half of the available non–participants due to computational constraints.

Table 4: Average ATT After Lock–In Period

QST00		
	Men	Women
Stratum 1	0.028 (0.016)*	0.070 (0.020)***
Stratum 2	0.111 (0.031)***	0.130 (0.034)***
Stratum 3	0.092 (0.026)***	0.025 (0.032)

MST00		
	Men	Women
Stratum 1	0.014 (0.019)	0.024 (0.019)
Stratum 2	0.027 (0.025)	0.143 (0.042)***
Stratum 3	0.063 (0.025)**	0.038 (0.032)

ST8092		
	Men	Women
Stratum 1	0.043 (0.029)	0.025 (0.034)
Stratum 2	0.062 (0.027)**	0.074 (0.036)**
Stratum 3	0.030 (0.028)	0.071 (0.026)***

Notes: QST00 refers to the qualification variant of short–term training administered in the early 2000s and MST00 to the monitoring variant. ST8092 denotes short–term training administered between 1980 and 1992. Average of the monthly treatment effects from month zero (program start) until the end of the observation period, i.e. until month 36 in the first stratum, month 30 in the second, and month 18 in the third for QST00 and MST00 programs, and until month 48 for ST8092 programs. *** = statistically significant at 1%, ** = at 5%, * = at 10%. Bootstrapped standard errors based on 250 replications.

Table 5: Average ATT Including Lock–In Period

QST00		
	Female=0	Female=1
Stratum 1	0.019 (0.015)	0.056 (0.018)***
Stratum 2	0.099 (0.027)***	0.104 (0.030)***
Stratum 3	0.080 (0.022)***	0.012 (0.025)

MST00		
	Female=0	Female=1
Stratum 1	0.008 (0.017)	0.015 (0.018)
Stratum 2	0.021 (0.022)	0.123 (0.037)***
Stratum 3	0.052 (0.021)**	0.028 (0.026)

ST8092		
	Men	Women
Stratum 1	0.029 (0.027)	0.019 (0.031)
Stratum 2	0.049 (0.024)**	0.059 (0.033)*
Stratum 3	0.026 (0.026)	0.065 (0.023)***

Notes: QST00 refers to the qualification variant of short–term training administered in the early 2000s and MST00 to the monitoring variant. ST8092 denotes short–term training administered between 1980 and 1992. Average of the monthly treatment effects from month zero (program start) until the end of the observation period, i.e. until month 36 in the first stratum, month 30 in the second, and month 18 in the third for QST00 and MST00 programs, and until month 48 for ST8092 programs. *** = statistically significant at 1%, ** = at 5%, * = at 10%. Bootstrapped standard errors based on 250 replications.

Table 6: Cumulated Treatment Effects – Employment

QST00, Men			
	6 Months	12 Months	24 Months[a]
Stratum 1	-0.145 (0.090)	-0.015 (0.187)	0.262 (0.378)
Stratum 2	0.274 (0.139)**	1.005 (0.308)***	2.429 (0.657)***
Stratum 3	0.319 (0.108)***	0.799 (0.241)***	1.518 (0.409)***

QST00, Women			
	6 Months	12 Months	24 Months[a]
Stratum 1	-0.080 (0.083)	0.369 (0.191)*	1.197 (0.422)***
Stratum 2	-0.009 (0.139)	0.669 (0.313)**	2.419 (0.699)***
Stratum 3	-0.108 (0.089)	0.013 (0.254)	0.224 (0.482)

MST00, Men			
	6 Months	12 Months	24 Months[a]
Stratum 1	-0.142 (0.089)	-0.167 (0.191)	-0.175 (0.403)
Stratum 2	-0.029 (0.115)	0.142 (0.277)	0.378 (0.560)
Stratum 3	0.166 (0.096)*	0.557 (0.236)**	0.984 (0.393)**

MST00, Women			
	6 Months	12 Months	24 Months[a]
Stratum 1	-0.203 (0.102)**	-0.322 (0.230)	-0.044 (0.452)
Stratum 2	0.223 (0.162)	0.907 (0.404)**	2.590 (0.896)***
Stratum 3	0.038 (0.107)	0.265 (0.277)	0.533 (0.498)

ST8092, Men				
	6 Months	12 Months	24 Months	48 Months
Stratum 1	-0.448 (0.143)***	-0.343 (0.315)	-0.050 (0.631)	1.308 (1.274)
Stratum 2	-0.285 (0.118)**	-0.004 (0.300)	0.910 (0.627)	2.336 (1.168)**
Stratum 3	-0.042 (0.116)	0.161 (0.258)	0.522 (0.552)	1.271 (1.236)

ST8092, Women				
	6 Months	12 Months	24 Months	48 Months
Stratum 1	-0.150 (0.142)	0.039 (0.342)	0.289 (0.770)	0.917 (1.501)
Stratum 2	-0.302 (0.154)*	0.019 (0.389)	0.910 (0.844)	2.813 (1.595)*
Stratum 3	0.130 (0.104)	0.613 (0.245)**	1.675 (0.543)***	3.124 (1.122)***

Notes: QST00 refers to the qualification variant of short–term training administered in the early 2000s and MST00 to the monitoring variant. ST8092 denotes short–term training administered between 1980 and 1992. Sum of the monthly treatment effects from month zero (program start). In columns marked with a [a], the treatment effects are summed over 18 months in stratum 3. *** = statistically significant at 1%, ** = at 5%, * = at 10%. Bootstrapped standard errors based on 250 replications.

Table 7: Cumulated Treatment Effects – Participation in Long–Term Training

QST00, Men			
	6 Months	12 Months	24 Months[a]
Stratum 1	0.198 (0.050)***	0.525 (0.110)***	1.033 (0.211)***
Stratum 2	0.077 (0.068)	0.293 (0.146)**	0.669 (0.289)**
Stratum 3	0.056 (0.059)	0.186 (0.121)	0.383 (0.186)**

QST00, Women			
	6 Months	12 Months	24 Months[a]
Stratum 1	0.156 (0.049)***	0.471 (0.108)***	0.894 (0.193)***
Stratum 2	0.047 (0.082)	0.188 (0.189)	0.316 (0.319)
Stratum 3	0.114 (0.078)	0.366 (0.184)**	0.569 (0.282)**

MST00, Men			
	6 Months	12 Months	24 Months[a]
Stratum 1	0.588 (0.070)***	1.383 (0.157)***	2.627 (0.305)***
Stratum 2	0.556 (0.112)***	1.393 (0.238)***	2.422 (0.426)***
Stratum 3	0.383 (0.087)***	1.045 (0.200)***	1.503 (0.293)***

MST00, Women			
	6 Months	12 Months	24 Months[a]
Stratum 1	0.535 (0.089)***	1.403 (0.197)***	2.632 (0.371)***
Stratum 2	0.425 (0.150)***	1.092 (0.324)***	2.014 (0.556)***
Stratum 3	0.348 (0.110)***	0.945 (0.269)***	1.585 (0.440)***

ST8092, Men				
	6 Months	12 Months	24 Months	48 months
Stratum 1	0.339 (0.110)***	0.630 (0.196)***	1.242 (0.350)***	1.907 (0.541)***
Stratum 2	0.492 (0.104)***	1.034 (0.202)***	1.579 (0.320)***	2.119 (0.438)***
Stratum 3	0.328 (0.087)***	0.780 (0.185)***	1.366 (0.326)***	1.549 (0.420)***

ST8092, Women				
	6 Months	12 Months	24 Months	48 Months
Stratum 1	0.539 (0.125)***	1.093 (0.232)***	1.353 (0.359)***	1.274 (0.405)***
Stratum 2	0.495 (0.126)***	1.015 (0.255)***	1.456 (0.379)***	1.675 (0.473)***
Stratum 3	0.502 (0.094)***	1.244 (0.217)***	1.890 (0.366)***	2.188 (0.475)***

Notes: QST00 refers to the qualification variant of short–term training administered in the early 2000s and MST00 to the monitoring variant. ST8092 denotes short–term training administered between 1980 and 1992. Sum of the monthly treatment effects from month zero (program start). In columns marked with a [a], the treatment effects are summed over 18 months in stratum 3. *** = statistically significant at 1%, ** = at 5%, * = at 10%. Bootstrapped standard errors based on 250 replications.

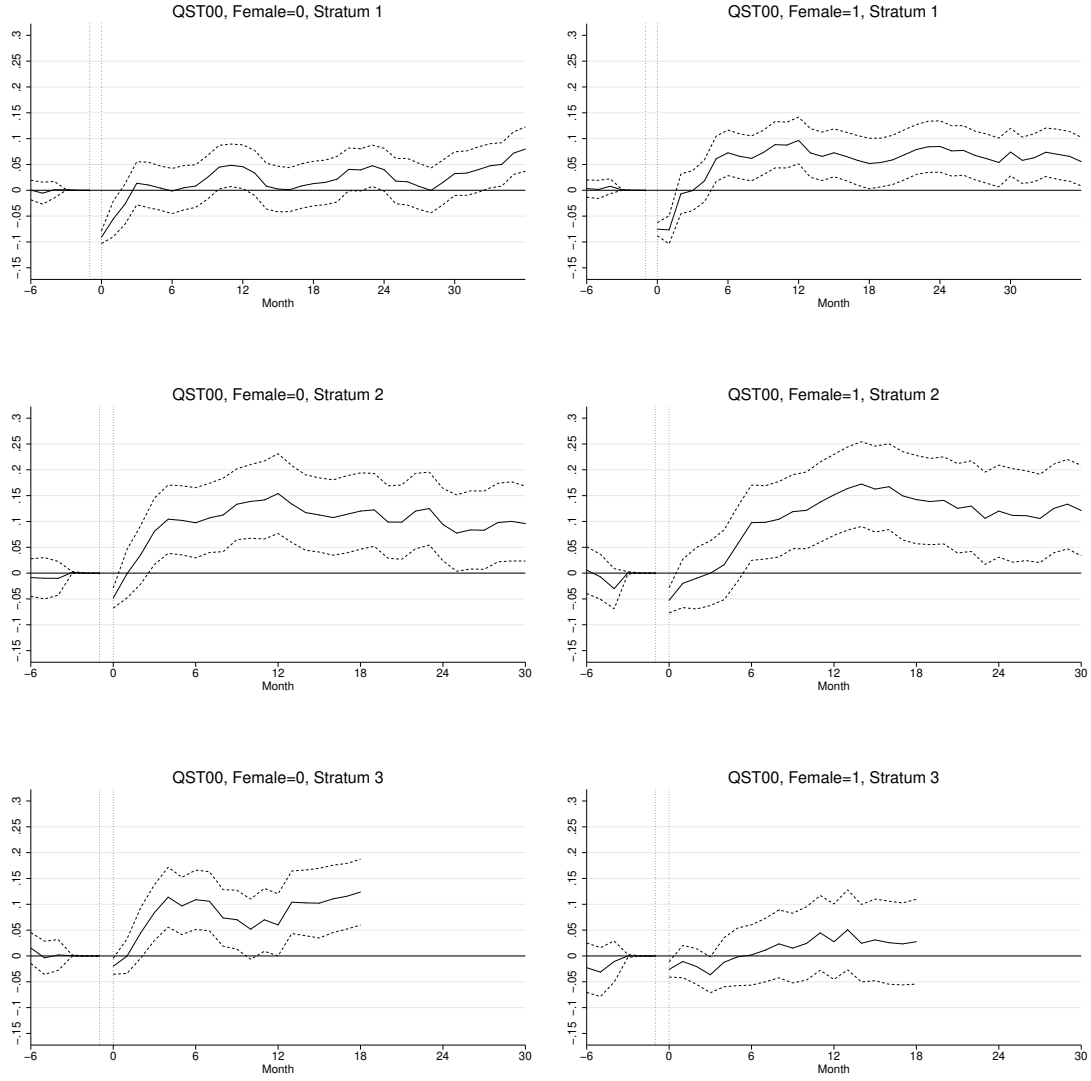
Table 8: Test of Heterogeneity of Employment Effects over Time

χ^2–Statistic (p–Value)		
Including Lock–In Period		
	Men	Women
Stratum 1	7.82 (0.729)	2.53 (0.996)
Stratum 2	14.75 (0.194)	10.85 (0.370)
Stratum 3	9.27 (0.597)	11.88 (0.373)
After Lock–In Period		
	Men	Women
Stratum 1	7.68 (0.741)	2.33 (0.997)
Stratum 2	15.27 (0.171)	11.08 (0.352)
Stratum 3	9.66 (0.561)	11.79 (0.380)
During Lock–In Period		
	Men	Women
Stratum 1	5.58 (0.900)	9.84 (0.545)
Stratum 2	9.04 (0.618)	4.72 (0.909)
Stratum 3	8.14 (0.700)	7.86 (0.726)

Notes: Test of equality of all year dummies in a regression of the individual treatment effects averaged over the months after program start on year dummies and elapsed unemployment duration. Empirical standard errors are calculated from bootstrap 250 resamples.

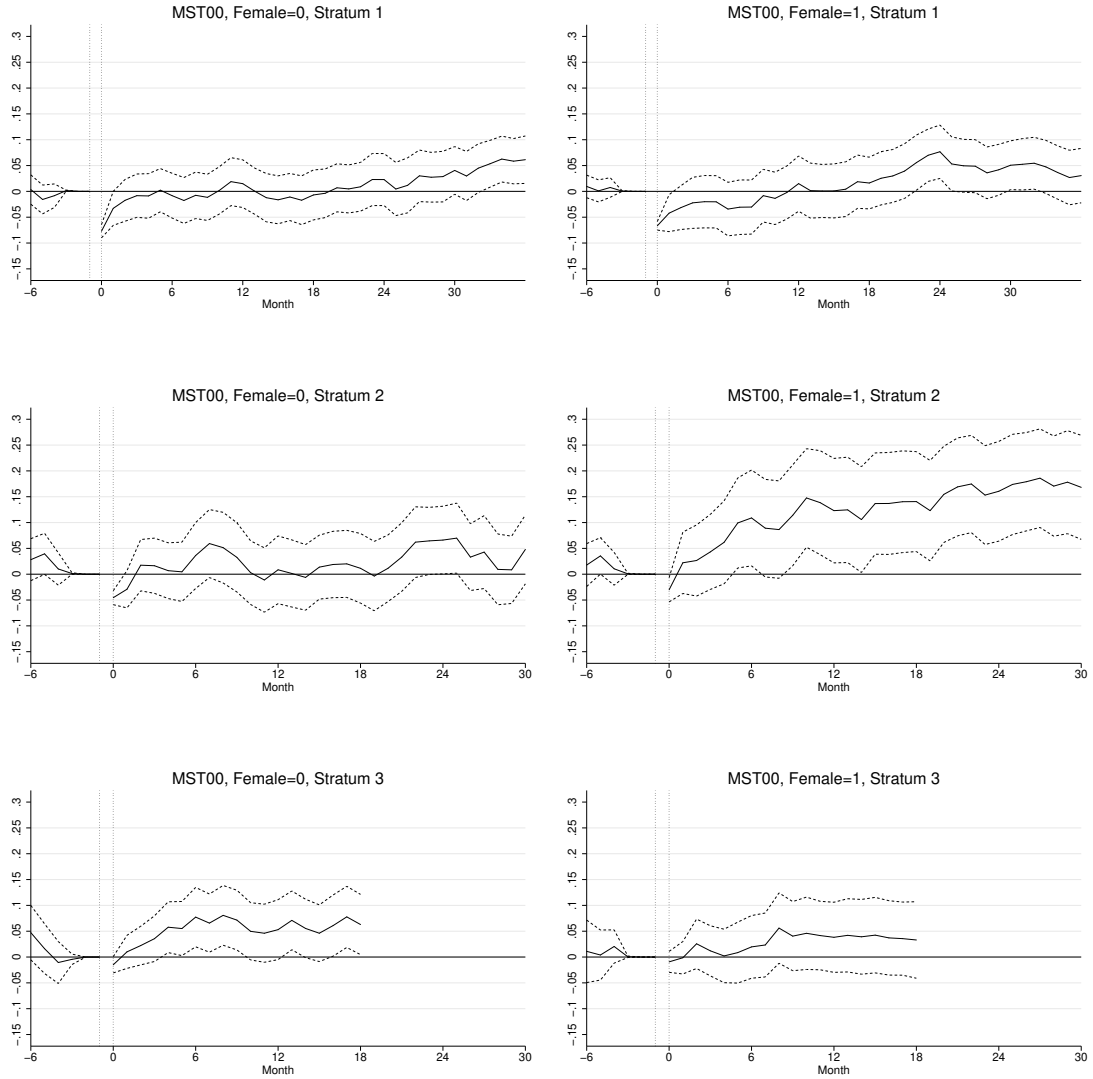
Figures

Figure 1: Average Treatment Effect on the Treated for QST00 – Employment



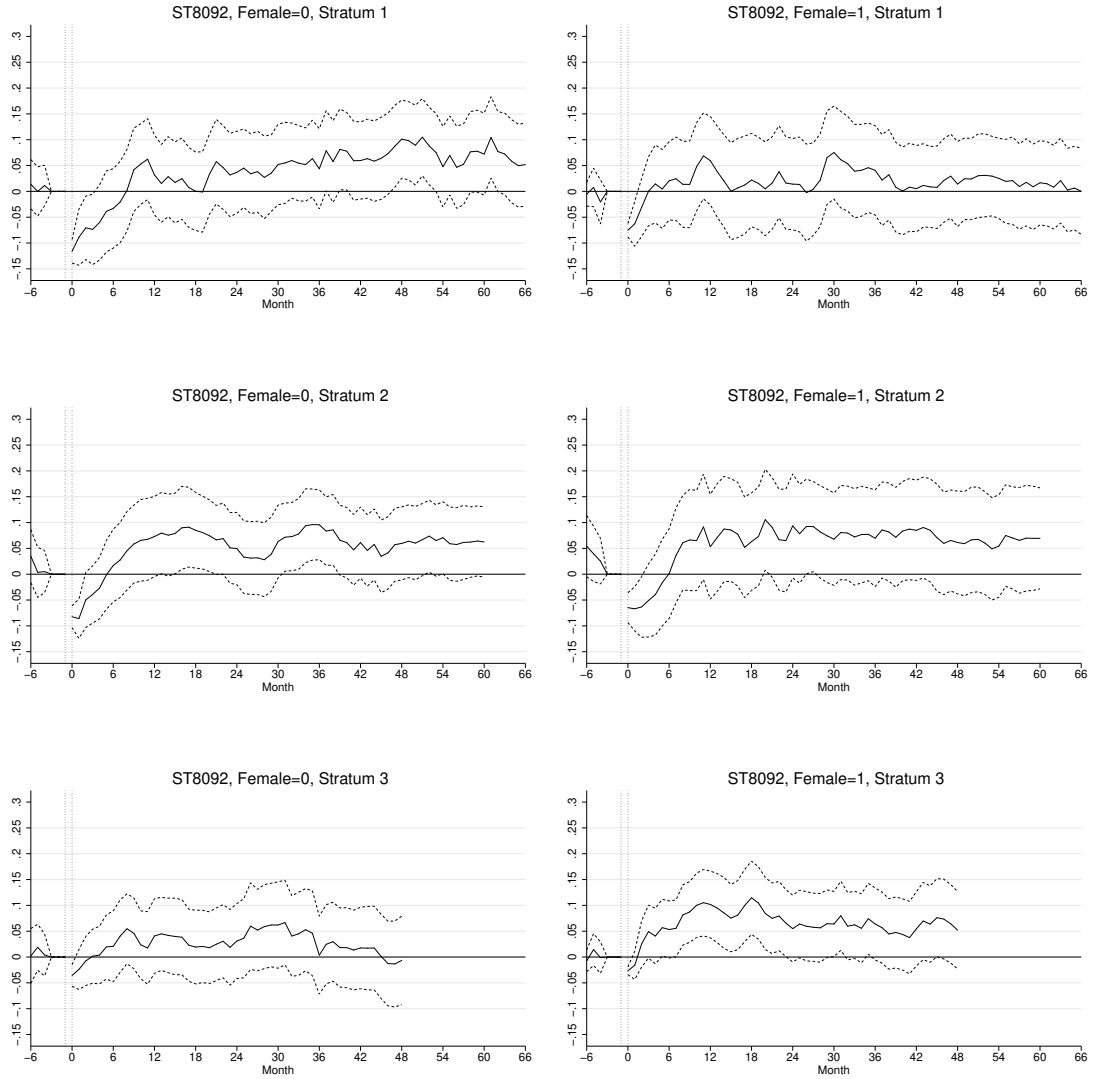
Notes: QST00 refers to the qualification variant of short–term training administered in the early 2000s. Difference in employment rates is measured on the ordinate, pre–unemployment (< 0) and post–treatment (≥ 0) months on the abscissa. Dashed lines are 95%-confidence intervals based on 250 bootstrap replications.

Figure 2: Average Treatment Effect on the Treated for MST00 – Employment



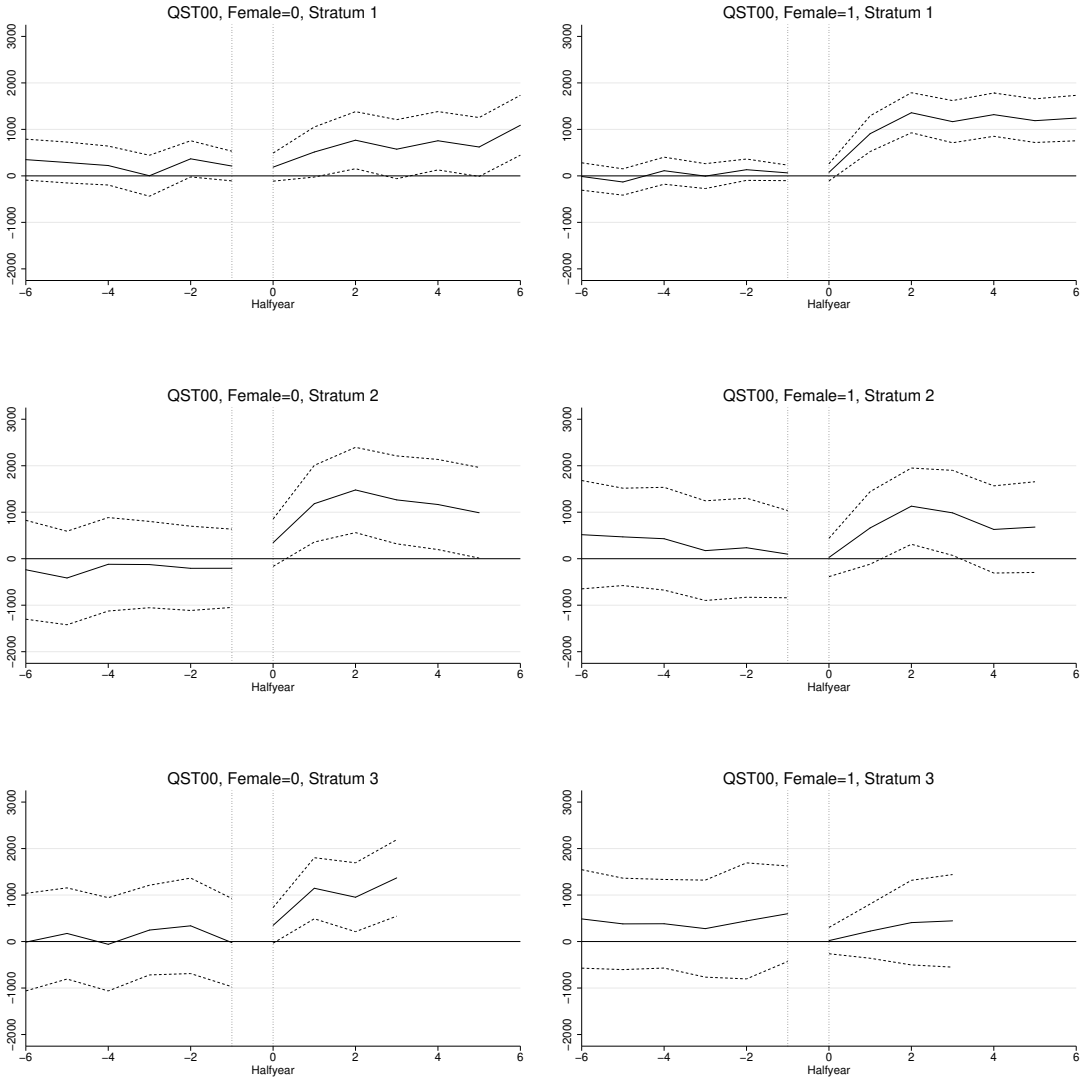
Notes: MST00 refers to the monitoring variant of short–term training administered in the early 2000s. Difference in employment rates is measured on the ordinate, pre–unemployment (< 0) and post–treatment (≥ 0) months on the abscissa. Dashed lines are 95%-confidence intervals based on 250 bootstrap replications.

Figure 3: Average Treatment Effect on the Treated for ST8092 – Employment



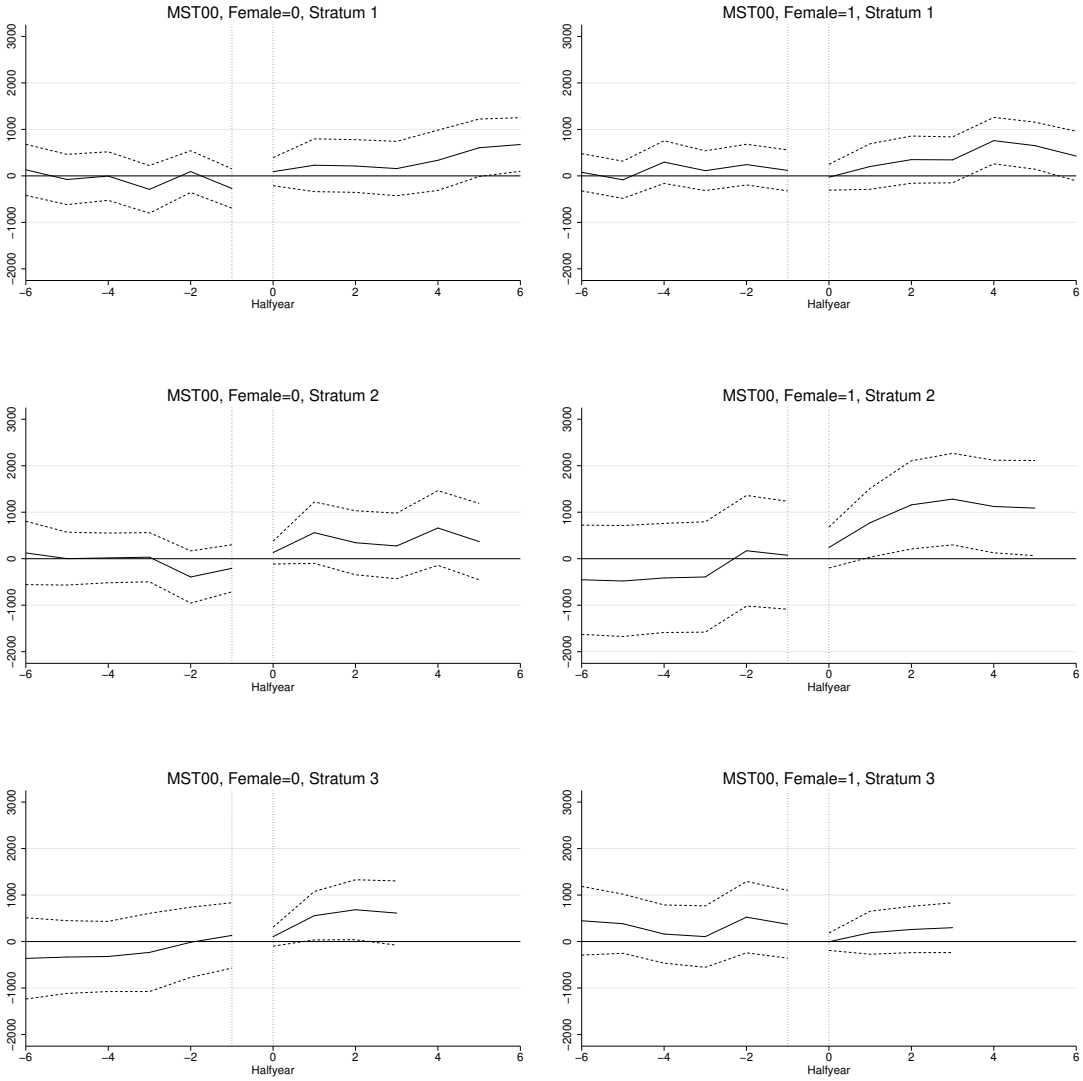
Notes: ST8092 refers to short–term training administered between 1980 and 1992. Difference in employment rates is measured on the ordinate, pre–unemployment (< 0) and post–treatment (≥ 0) months on the abscissa. Dashed lines are 95%-confidence intervals based on 250 bootstrap replications.

Figure 4: Average Treatment Effect on the Treated for QST00 – Earnings



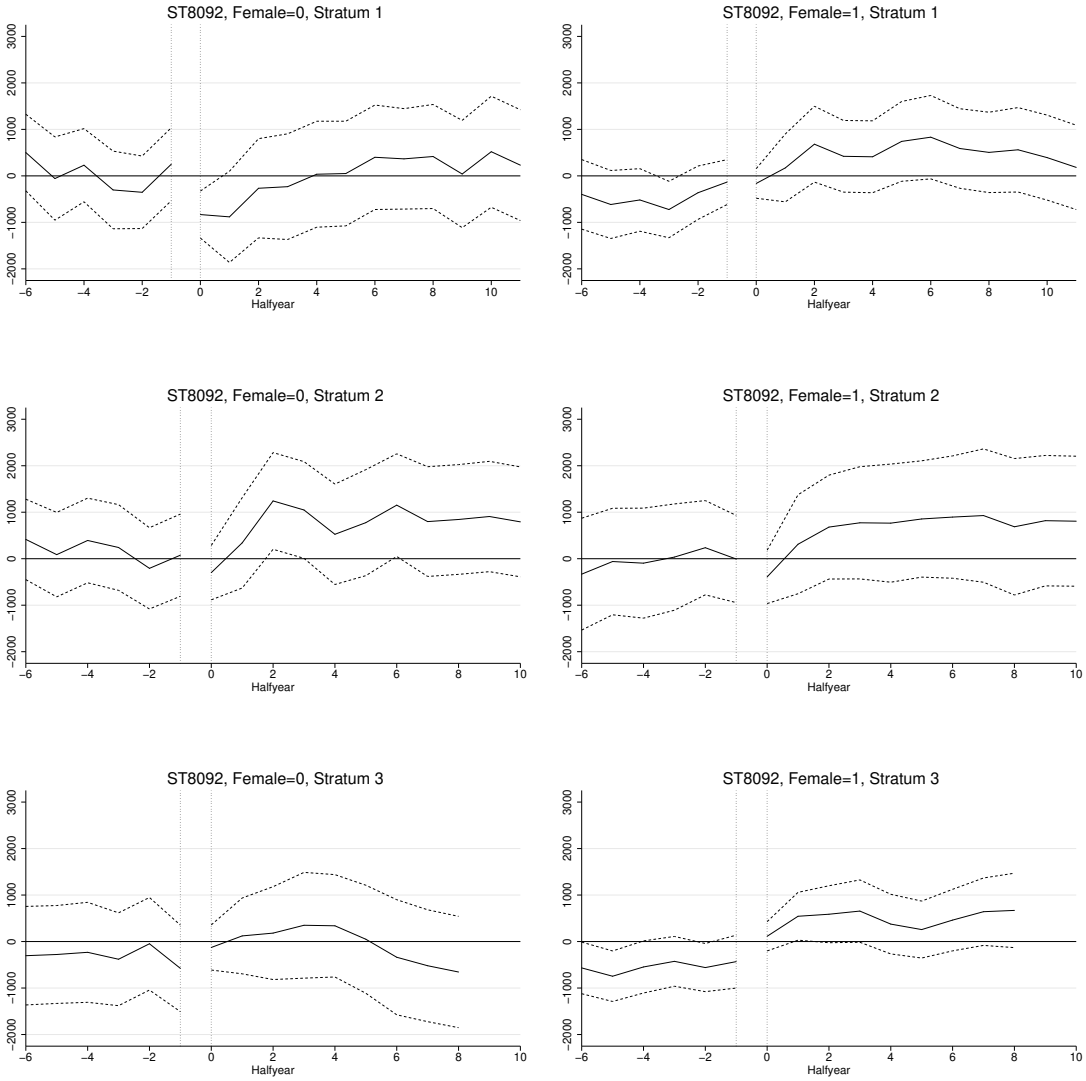
Notes: QST00 refers to the qualification variant of short–term training administered in the early 2000s. Difference in real earnings (in 1995 €) within a six–month period is measured on the ordinate, pre–unemployment (< 0) and post–treatment (≥ 0) six–month periods on the abscissa. Dashed lines are 95%-confidence intervals based on 250 bootstrap replications.

Figure 5: Average Treatment Effect on the Treated for MST00 – Earnings



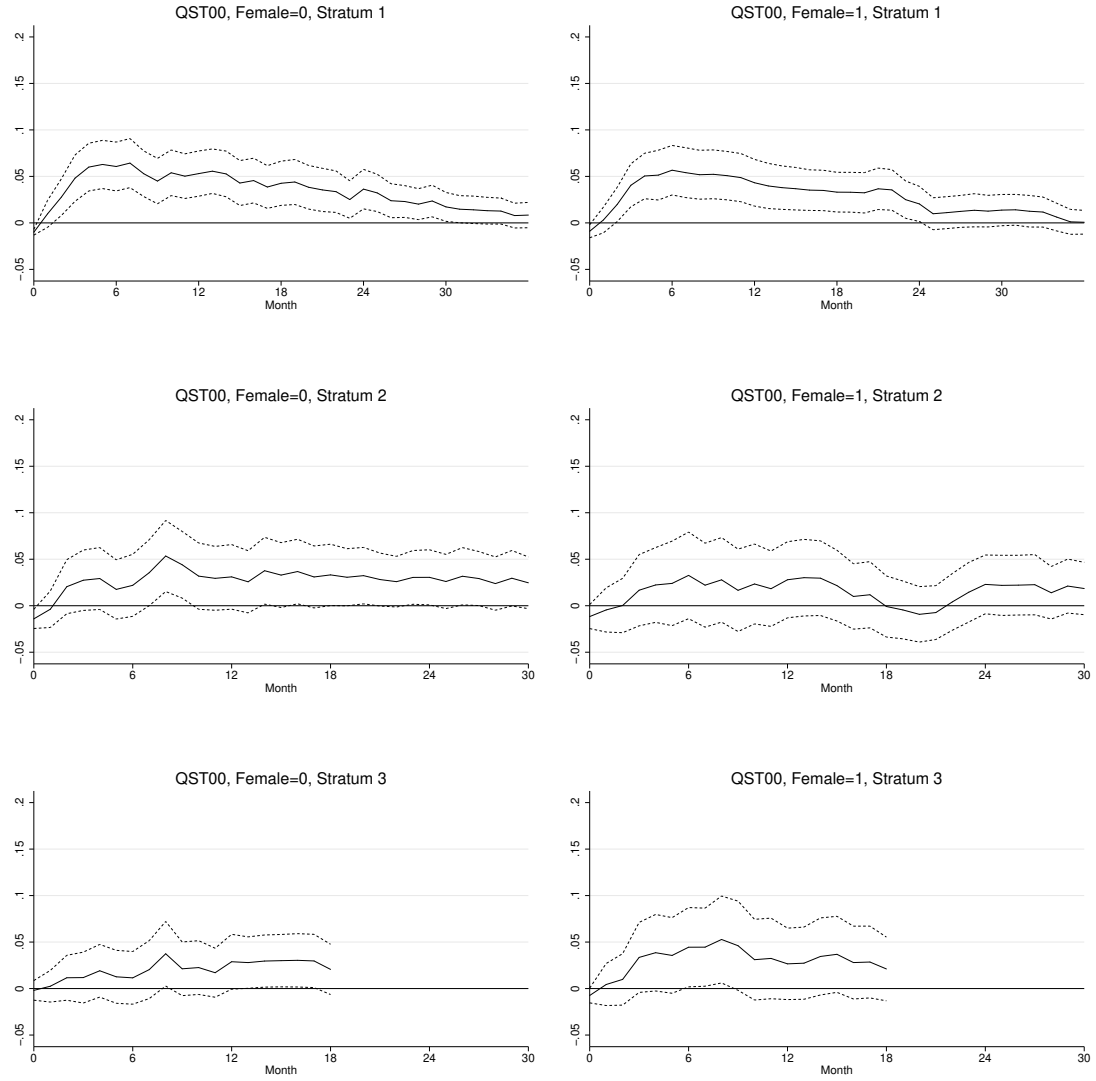
Notes: MST00 refers to the monitoring variant of short–term training administered in the early 2000s. Difference in real earnings (in 1995 €) within a six–month period is measured on the ordinate, pre–unemployment (< 0) and post–treatment (≥ 0) six–month periods on the abscissa. Dashed lines are 95%-confidence intervals based on 250 bootstrap replications.

Figure 6: Average Treatment Effect on the Treated for ST8092 – Earnings



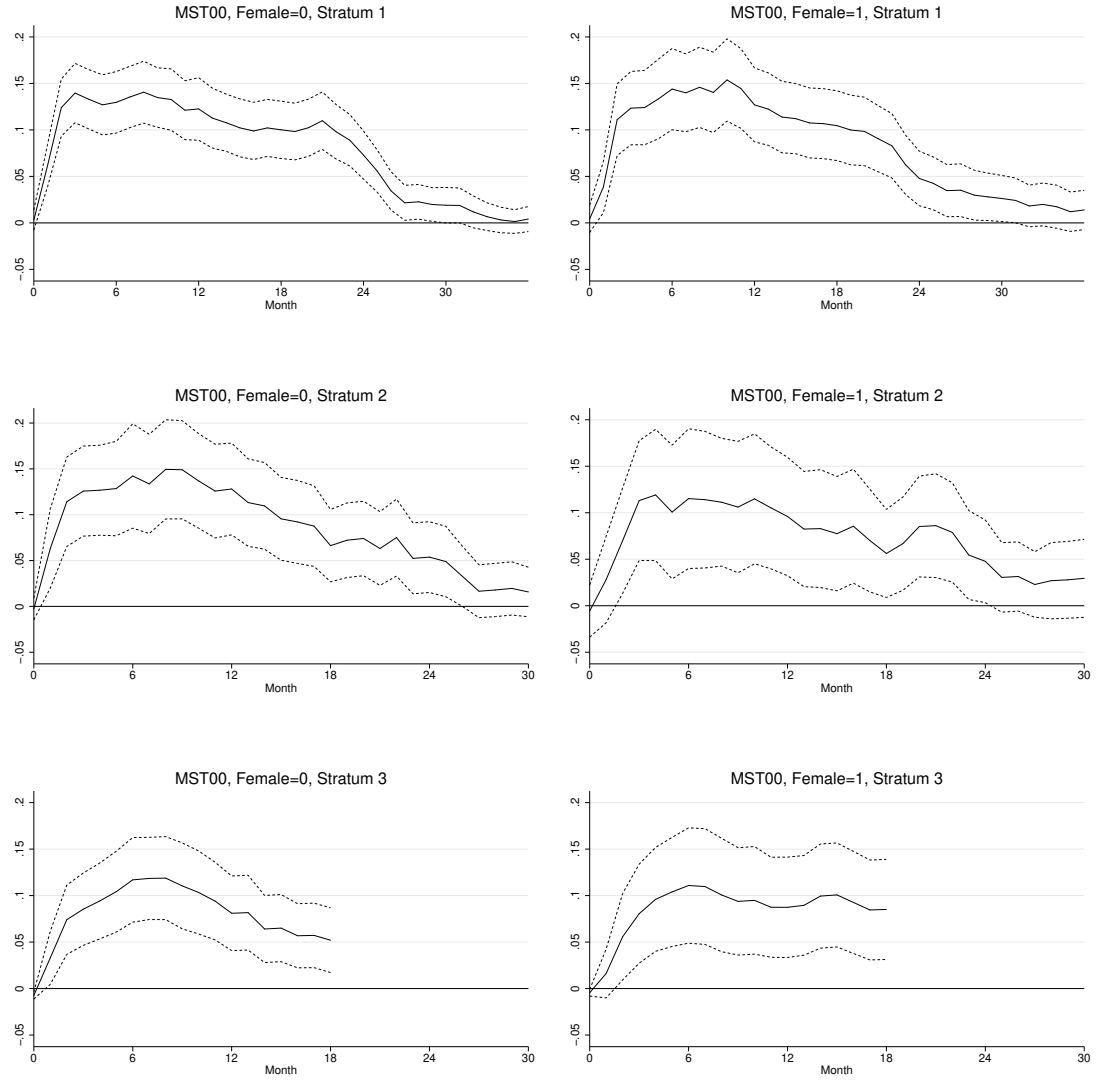
Notes: ST8092 refers to short–term training administered between 1980 and 1992. Difference in real earnings (in 1995 €) within a six–month period is measured on the ordinate, pre–unemployment (< 0) and post–treatment (≥ 0) six–month periods on the abscissa. Dashed lines are 95%-confidence intervals based on 250 bootstrap replications.

Figure 7: Average Treatment Effect on the Treated for QST00 – Participation in Long–Term Training



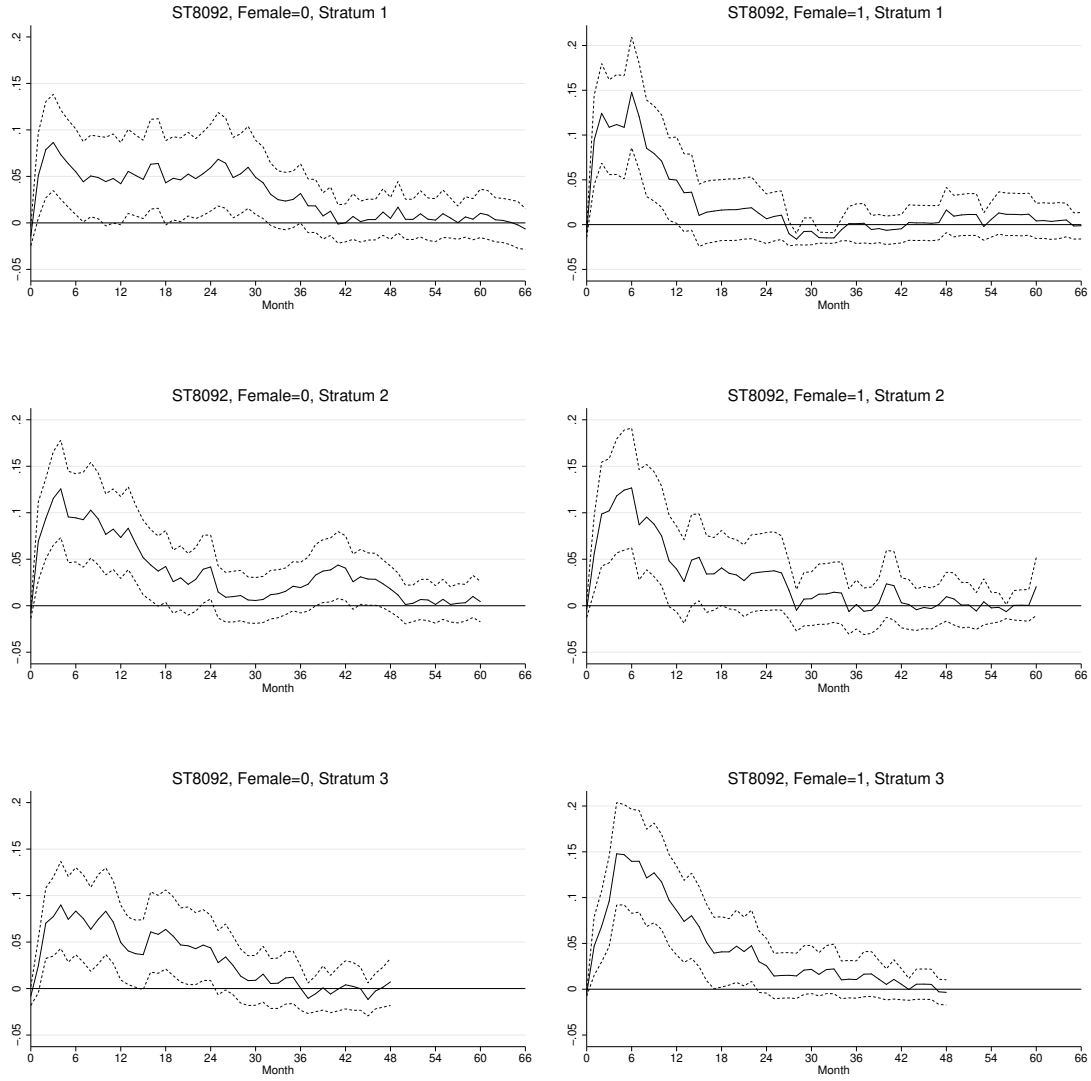
Notes: QST00 refers to the qualification variant of short–term training administered in the early 2000s. Difference in participation rates is measured on the ordinate, post–treatment months on the abscissa. Dashed lines are 95%-confidence intervals based on 250 bootstrap replications.

Figure 8: Average Treatment Effect on the Treated for MST00 – Participation in Long–Term Training



Notes: MST00 refers to the monitoring variant of short–term training administered in the early 2000s. Difference in participation rates is measured on the ordinate, post–treatment months on the abscissa. Dashed lines are 95%-confidence intervals based on 250 bootstrap replications.

Figure 9: Average Treatment Effect on the Treated for ST8092 – Participation in Long–Term Training



Notes: ST8092 refers to short–term training administered between 1980 and 1992. Difference in participation rates is measured on the ordinate, post–treatment months on the abscissa. Dashed lines are 95%-confidence intervals based on 250 bootstrap replications.

On–line Appendix to "Déjà Vu? Short–Term Training in Germany 1980–1992 and 2000–2003"

By Bernd Fitzenberger, Olga Orlanski, Aderonke Osikominu, and Marie Paul

Table 1: Means of Important Variables for the 2000–2003 Treatment Sample, QST00

Label	Men	Women
Personal Attributes		
Age at start of unemployment $\geq$ 25 and $\leq$29	0.169	0.151
Age at start of unemployment $\geq$ 30 and $\leq$34	0.209	0.154
Age at start of unemployment $\geq$ 35 and $\leq$39	0.227	0.212
Age at start of unemployment $\geq$ 40 and $\leq$44	0.161	0.203
Age at start of unemployment $\geq$ 45 and $\leq$49	0.142	0.188
Age at start of unemployment $\geq$ 50 and $\leq$53	0.092	0.092
Non–German nationality*	0.157	0.080
Vocational training degree*	0.568	0.637
University or technical college degree*	0.060	0.060
Last Employment		
Elementary occupations, skilled agriculture, fishery workers	0.192	0.101
Craftsmen, machine operators and related	0.445	0.145
Service workers	0.077	0.256
Clerks	0.125	0.285
Technicians and associate professionals	0.101	0.136
Professionals and managers	0.059	0.077
Manufacturing	0.269	0.173
Construction, agriculture, forestry, fishing	0.139	0.041
Trade and transport	0.301	0.342
Financial, renting and business	0.174	0.178
Other Services	0.117	0.266
Employment History		
Employed in month 12 before start of unemployment	0.756	0.764
Employed in month 24 before start of unemployment	0.636	0.625

Notes: For rows marked with a *, missings are included in a separate category not shown in the table.

Table 2: Means of Important Variables for the 2000–2003 Treatment Sample, MST00

Label	Men	Women
Personal Attributes		
Age at start of unemployment $\geq$ 25 and $\leq$29	0.190	0.158
Age at start of unemployment $\geq$ 30 and $\leq$34	0.244	0.187
Age at start of unemployment $\geq$ 35 and $\leq$39	0.230	0.215
Age at start of unemployment $\geq$ 40 and $\leq$44	0.157	0.195
Age at start of unemployment $\geq$ 45 and $\leq$49	0.102	0.155
Age at start of unemployment $\geq$ 50 and $\leq$53	0.078	0.090
Non–German nationality*	0.188	0.107
Vocational training degree*	0.519	0.532
University or technical college degree*	0.042	0.063
Last Employment		
Elementary occupations, skilled agriculture, fishery workers	0.205	0.127
Craftsmen, machine operators and related	0.478	0.148
Service workers	0.084	0.269
Clerks	0.103	0.239
Technicians and associate professionals	0.082	0.137
Professionals and managers	0.048	0.079
Manufacturing	0.256	0.188
Construction, agriculture, forestry, fishing	0.194	0.03
Trade and transport	0.288	0.331
Financial, renting and business	0.171	0.174
Other Services	0.091	0.276
Employment History		
Employed in month 12 before start of unemployment	0.714	0.711
Employed in month 24 before start of unemployment	0.599	0.583

Notes: For rows marked with a *, missings are included in a separate category not shown in the table.

Table 3: Means of Important Variables for the 1980–1992 Treatment Sample, ST8092

Label	Men	Women
Personal Attributes		
Age at start of unemployment $\geq$ 25 and $\leq$29	0.260	0.306
Age at start of unemployment $\geq$ 30 and $\leq$34	0.211	0.212
Age at start of unemployment $\geq$ 35 and $\leq$39	0.164	0.182
Age at start of unemployment $\geq$ 40 and $\leq$44	0.180	0.133
Age at start of unemployment $\geq$ 45 and $\leq$49	0.120	0.107
Age at start of unemployment $\geq$ 50 and $\leq$53	0.064	0.059
Non–German nationality*	0.115	0.046
Vocational training degree*	0.747	0.713
University or technical college degree*	0.044	0.042
Last Employment		
Full–time blue collar employee	0.732	0.328
Full–time white collar employee	0.250	0.492
Apprentice, worker at home, part–time working	0.018	0.180
Production oriented services, trade, banking	0.317	0.337
Metal, vehicles, electronics	0.175	0.123
Consumer oriented, organizational, and social services	0.149	0.315
Construction, agriculture	0.149	0.030
Light industry	0.097	0.140
Basic materials	0.113	0.060
Employment History		
Employed in month 12 before start of unemployment	0.690	0.790
Employed in month 24 before start of unemployment	0.658	0.718

Notes: For rows marked with a *, missings are included in a separate category not shown in the table.

Table 4: Variable Definitions for the 2000–2003 Sample

Label	Definition
Personal Attributes	
female	1 if female, 0 otherwise
agegroup	age in 6 groups
foreigner	1 if citizenship is not German, 0 otherwise or missing
qualification	1 no degree or missing, 2 vocational training degree, 3 university or technical college degree
schooling	1 no schooling degree or missing, 2 Hauptschulabschluss or Mittlere Reife /Fachoberschule (degrees reached after completion of the 9th or 10th grade), 3 Fachhochschulreife or Abitur/Hochschulreife (degrees reached after completion of the 12th or 13th grade)
health	1 no health problems mentioned, 2 health problems, but considered without impact on placement, 3 health problems considered to have an impact on placement
pasthealth	same categories as health, but referring to the past two years before the beginning of the unemployment spell
disabled	1 if disabled, 0 no disability mentioned
married	1 missing, 2 married, 3 not married
child	1 if at least one child, 0 otherwise or missing
youngchild	1 if at least one child younger than 10 years, 0 otherwise or missing
Last Employment	
occupation	occupation of last employment in 7 categories
industry	industry of last employment in 6 categories
endlastjob	2 termination of last job by employer, 3 by employee, 4 limited in time, 5 other and missing
waged	daily wage in the last job(s) before the beginning of the unemployment spell
ddssec	ddsec is 1 if earnings are within the social security thresholds
lnwage	log(waged) interacted with ddssec
parttime	1 if the person worked less than full–time in the last employment, 0 otherwise

<continued on next page>

Label	Definition
onlyparttime	1 if information available that only part–time job is desired, 0 otherwise
Employment and Program History	
problemgroup	1 if participated in a program with a social work component (i.e. in a program supporting rehabilitation and socio-professional integration) within the last three years, 0 otherwise
pasttreatnotcompl	1 if a benefit spell was terminated as a result of dropping out of an active labor market program in the past three years, 0 otherwise
penalty	1 if the unemployed had a period of disqualification from benefits within the last three years, 0 otherwise
motivationlack	1 if within the last three years a benefit or job search spell was terminated or suspended because the person has failed to comply with the rules, e.g. if he/she has missed out on a meeting with the caseworker or has not cooperated in a sufficient way; 0 otherwise
countemp, countub, countua, countsub, countoos, countcon	number of days within the last three years before the beginning of unemployment spent in regular employment, receiving unemployment benefits, unemployment assistance, subsistance payment, out of sample, in contact with the labor office, respectively
dcount...	1 if the respective count variable is larger than 0, 0 otherwise
demp6, demp12, demp24, demp6_12, demp12_24	1 if in regular employment 6, 12, 24, 6 and 12 and 12 and 24 months, respectively, before the beginning of the unemployment spell
claimg	remaining claim on unemployment benefit in four categories
Regional Information	
area	German Bundesländer aggregated into 6 categories. 1 SH, NI, HB, HH; 2 NW, 3 HE, RP, SL; 4 BY, BW; 5 MV, BB, BE; 6 SN, ST, TH
region	classification of the districts of residence according to local labor market conditions in 5 groups

<continued on next page>

Label	Definition
Calendar Time of Entry into Unemployment	
quarter	calendar quarter of the end of the last employment (enumerates the six quarters of our inflow sample)

Notes: If not mentioned otherwise, variables are defined relative to the beginning of the time window of elapsed unemployment duration. Variables in categories are used as dummies, i.e. agegroup1 equals 1 if agegroup takes the value 1 and 0 otherwise. If there are only few observations with missing values on a particular variable, these are subsumed into one of the substantive categories.

Table 5: Estimated Coefficients of the Propensity Scores for the 2000–2003 Sample

Participation Probit for QST00, Males

	Stratum 1	Stratum 2	Stratum 3
agegroup1		-0.166 (0.098)*	-0.160 (0.120)
agegroup12	0.001 (0.041)		
agegroup2		-0.123 (0.088)	-0.090 (0.102)
agegroup4		-0.002 (0.090)	-0.082 (0.107)
agegroup5		-0.080 (0.105)	0.214 (0.102)**
agegroup6		0.140 (0.106)	0.145 (0.113)
area2		-0.082 (0.081)	-0.151 (0.085)*
area3		-0.008 (0.097)	-0.125 (0.113)
area4		-0.129 (0.112)	-0.319 (0.130)**
child	0.116 (0.048)**	0.087 (0.067)	0.179 (0.071)**
claimg0		-0.031 (0.105)	0.043 (0.095)
claimg1	0.130 (0.103)		
claimg2	0.116 (0.088)	-0.124 (0.085)	-0.072 (0.101)
claimg3	0.096 (0.098)		
claimg34		-0.383 (0.116)***	-0.210 (0.119)*
claimg4	0.169 (0.108)		
countcon	0 (0.000)	0 (0.000)	0 (0.000)*
countemp	0 (0.000)**	0.001 (0.000)***	0 (0.000)
dcountcon	0.068 (0.053)	0.021 (0.079)	0.029 (0.086)
dcountoos		0.011 (0.078)	-0.148 (0.086)*
dcountsub	0.214 (0.071)***		
ddssec	0.607 (0.242)**		
demp12_24	0.195 (0.074)***	-0.045 (0.115)	0.055 (0.144)
demp24	-0.150 (0.076)**	0.010 (0.121)	0.009 (0.144)
demp6	0.026 (0.063)	-0.033 (0.097)	0.292 (0.114)**
endlastjob2	0.045 (0.046)		
endlastjob4	0.236 (0.064)***		
industry3		-0.162 (0.086)*	-0.267 (0.095)***
industry5		0.095 (0.078)	-0.192 (0.099)*
industry6		-0.027 (0.097)	-0.151 (0.104)
lnwaged	-0.135 (0.053)**		
married2	0.165 (0.044)***	0.167 (0.065)**	0.281 (0.071)***
motivationlack	0.105 (0.055)*		
occupation1	0.102 (0.073)		
occupation3	-0.013 (0.065)		
occupation5	0.243 (0.080)***		
occupation6	0.139 (0.086)		
occupation7	-0.002 (0.105)		
problemgroup	0.224 (0.092)**		
quarter1	-0.199 (0.054)***	-0.196 (0.105)*	-0.154 (0.104)
quarter2		0.029 (0.104)	-0.211 (0.117)*
quarter3		0.091 (0.095)	-0.182 (0.110)*
quarter4	-0.120 (0.051)**	-0.062 (0.094)	-0.122 (0.100)
quarter5	-0.086 (0.049)*		
quarter6		0.121 (0.093)	-0.115 (0.101)
region2		-0.237 (0.097)**	-0.092 (0.096)
region4		0.090 (0.104)	0.157 (0.115)
region5		0.038 (0.095)	0.225 (0.115)*
schooling3	0.143 (0.060)**		
youngchild	-0.048 (0.062)		
intercept	-2.633 (0.169)***	-2.354 (0.231)***	-1.961 (0.265)***

<continued on next page>

Table 5: Estimated Coefficients of the Propensity Scores for the 2000–2003 Sample

N	21538	8558	5333
	Participation Probit for MST00, Males		
	Stratum 1	Stratum 2	Stratum 3
agegroup1		0.063 (0.095)	
agegroup2		0.231 (0.080)***	
agegroup56	-0.033 (0.050)	0.006 (0.098)	
child		0.189 (0.072)***	0.115 (0.068)*
claimg0	-0.027 (0.079)	-0.059 (0.215)	0.142 (0.097)
claimg0_dcountoos		0.370 (0.206)*	
claimg1	0.053 (0.084)	-0.040 (0.195)	-0.085 (0.108)
claimg1_dcountoos		0.286 (0.204)	
claimg2		0.221 (0.107)**	
claimg34	0.008 (0.047)		-0.124 (0.117)
countub			-0 (0.000)
dcountcon	0.091 (0.054)*		0.129 (0.084)
dcountoos	0.064 (0.042)	-0.169 (0.091)*	
dcountsub			0.237 (0.112)**
dcountub	-0.135 (0.046)***		
demp12		0.005 (0.151)	0.029 (0.138)
demp24		-0.029 (0.080)	0.054 (0.073)
demp6_12	0.124 (0.050)**	0.106 (0.153)	-0.127 (0.133)
endlastjob2	0.102 (0.046)**		
endlastjob3	-0.172 (0.117)		
endlastjob4	0.135 (0.071)*		
foreigner		-0.291 (0.096)***	
health2		0.284 (0.127)**	
health3		-0.110 (0.135)	
industry2	0.520 (0.171)***		
industry3	0.314 (0.171)*		-0.093 (0.099)
industry4	0.432 (0.170)**		-0.067 (0.085)
industry5	0.500 (0.173)***		-0.150 (0.103)
industry6	0.350 (0.180)*		-0.321 (0.121)***
married2	0.129 (0.042)***	0.220 (0.069)***	0.234 (0.071)***
motivationlack	0.109 (0.055)**		-0.027 (0.078)
pasthealth1			0.346 (0.123)***
pasthealth2		-0.063 (0.172)	
pasthealth3		0.312 (0.149)**	
penalty			0.219 (0.126)*
qualification1	0.107 (0.041)***	0.182 (0.068)***	0.041 (0.066)
quarter1	0 (0.070)		
quarter3	0.008 (0.075)		
quarter4	-0.056 (0.071)		0.098 (0.088)
quarter5	-0.034 (0.070)		0.186 (0.084)**
quarter6	0.161 (0.071)**		0.141 (0.092)
region2	0.536 (0.079)***		
region3	0.278 (0.074)***	-0.037 (0.086)	-0.140 (0.082)*
region4		-0.194 (0.126)	-0.258 (0.121)**
region5	0.122 (0.080)	-0.169 (0.102)*	-0.283 (0.101)***
schooling3	0.090 (0.057)		
intercept	-2.961 (0.207)***	-2.406 (0.170)***	-2.176 (0.206)***
N	21510	8514	5336

<continued on next page>

Table 5: Estimated Coefficients of the Propensity Scores for the 2000–2003 Sample

Participation Probit for QST00, Females

	Stratum 1	Stratum 2	Stratum 3
agegroup12	-0.113 (0.048)**		-0.228 (0.099)**
agegroup4		0.193 (0.080)**	
agegroup56	-0.021 (0.053)	0.155 (0.081)*	0.158 (0.094)*
child	0.116 (0.045)**	0.223 (0.068)***	0.197 (0.088)**
claimg0	-0.042 (0.127)	0.187 (0.122)	0.242 (0.118)**
claimg2	0.166 (0.095)*	0.254 (0.109)**	0.143 (0.126)
claimg34	0.173 (0.100)*	0.126 (0.149)	0.153 (0.141)
claimg34_married2		0.242 (0.170)	
countcon	0 (0.000)	0.001 (0.000)*	0 (0.000)
countemp	0 (0.000)	0 (0.000)	-0 (0.001)
countoos		0.001 (0.000)	0 (0.000)
dcountcon	0.135 (0.057)**		
dcountoos	-0.084 (0.054)		
dcountua		0.170 (0.108)	
dcountub		0.087 (0.088)	
ddssec	-4.380 (1.263)***		
demp12	0.049 (0.133)	0.075 (0.188)	-0.178 (0.277)
demp12_24	-0.037 (0.076)	0.072 (0.120)	0.239 (0.159)
demp6	0.027 (0.092)	0.027 (0.126)	0.235 (0.159)
demp6_12	-0.015 (0.136)	0.034 (0.198)	0.145 (0.275)
endlastjob2	0.227 (0.056)***	0.375 (0.084)***	
endlastjob3	0.291 (0.075)***	0.139 (0.123)	
endlastjob4	0.081 (0.072)	0.122 (0.113)	
foreigner		-0.301 (0.115)***	
health2	-0.271 (0.143)*		
health3	0.079 (0.139)		0.234 (0.117)**
industry3	0.282 (0.118)**		
industry4	0.062 (0.058)	0.172 (0.085)**	
industry5	0.068 (0.067)	0.044 (0.102)	
industry6	0.015 (0.061)	0.030 (0.090)	
lnwaged	2.241 (0.660)***		
lnwagedsq	-0.281 (0.086)***		
married2	0.222 (0.044)***	0.200 (0.074)***	0.455 (0.083)***
motivationlack		-0.082 (0.085)	
pasthealth2	0.326 (0.138)**		
pasthealth3	-0.041 (0.147)		
pasttreatnotcompl		0.155 (0.339)	
penalty		0.332 (0.159)**	
problemgroup	0.240 (0.117)**		
region2	0.075 (0.058)		
region3		0.026 (0.096)	
region4		0.086 (0.123)	
region5		0.114 (0.100)	
intercept	-2.408 (0.224)***	-3.386 (0.470)***	-2.733 (0.521)***
N	14385	7284	5105

<continued on next page>

Table 5: Estimated Coefficients of the Propensity Scores for the 2000–2003 Sample

Participation Probit for MST00, Females

	Stratum 1	Stratum 2	Stratum 3
agegroup1		-0.175 (0.117)	
agegroup2		-0.207 (0.106)*	
agegroup4			0.261 (0.115)**
agegroup5			0.280 (0.116)**
agegroup56	-0.262 (0.098)***	-0.169 (0.123)	
agegroup6			0.372 (0.117)***
area3		-0.214 (0.111)*	
child	0.213 (0.055)***	0.104 (0.089)	
claimg0	-0.045 (0.117)	0.324 (0.147)**	0.178 (0.117)
claimg1	-0.070 (0.109)	0.105 (0.126)	-0.053 (0.132)
claimg3	0.098 (0.101)	-0.143 (0.133)	
claimg34			-0.066 (0.129)
claimg3_dcountoos	-0.114 (0.116)		
claimg4	0.506 (0.140)***	0.023 (0.164)	
claimg4_dcountoos	-0.380 (0.182)**		
countoos		0 (0.000)	-0 (0.000)
countub		-0.001 (0.001)	-0.001 (0.000)
dcountcon	0.066 (0.062)	0.073 (0.094)	
dcountoos	0.154 (0.092)*		
dcountsub			0.264 (0.163)
demp12	0.059 (0.090)	-0.174 (0.242)	-0.288 (0.127)**
demp12_24	0.045 (0.131)		
demp24	0.037 (0.130)	0.021 (0.156)	-0.120 (0.141)
demp6	0.095 (0.087)		
demp6_12		0.295 (0.245)	
endlastjob2	0.147 (0.065)**	0.161 (0.089)*	
endlastjob3	0.229 (0.089)**		
endlastjob4	0.138 (0.080)*	0.189 (0.116)	
lncountemp	-0.087 (0.092)	-1.548 (1.464)	-0.626 (1.361)
lncountempsq		0.135 (0.131)	0.062 (0.119)
married2	0.201 (0.054)***	0.271 (0.080)***	0.193 (0.087)**
onlyparttime	-0.120 (0.066)*		
parttime		-0.005 (0.079)	-0.073 (0.084)
qualification1	0.148 (0.053)***		0.173 (0.083)**
quarter1	0.295 (0.087)***	0.282 (0.140)**	
quarter2	0.235 (0.092)**	0.263 (0.149)*	0.141 (0.167)
quarter3	0.275 (0.088)***	0.375 (0.136)***	0.169 (0.162)
quarter4			0.317 (0.151)**
quarter5	0.350 (0.083)***	0.225 (0.137)*	0.232 (0.151)
quarter6	0.231 (0.091)**	0.413 (0.136)***	0.337 (0.153)**
region2	0.457 (0.100)***	0.312 (0.132)**	0.460 (0.169)***
region3	0.309 (0.091)***	0.426 (0.103)***	0.355 (0.156)**
region4		0.129 (0.154)	
region5	0.150 (0.097)		0.099 (0.172)
schooling3	0.084 (0.064)		
youngchild		-0.232 (0.121)*	0.353 (0.105)***
intercept	-2.563 (0.540)***	1.394 (4.026)	-1.093 (3.846)
N	14173	7196	5090

Table 6: Results of Smith and Todd (2005) Balancing Test for the 2000–2003 Sample

QST00, Males, Cubic of Pscore				
	P-values>.1	P-values>.05	P-values>.01	Regressors
Stratum 1	28	29	30	30
Stratum 2	27	30	31	31
Stratum 3	28	30	31	31
QST00, Males, Quartic of Pscore				
	P-values>.1	P-values>.05	P-values>.01	Regressors
Stratum 1	28	28	30	30
Stratum 2	26	27	31	31
Stratum 3	31	31	31	31
MST00, Males, Cubic of Pscore				
	P-values>.1	P-values>.05	P-values>.01	Regressors
Stratum 1	26	28	28	28
Stratum 2	23	23	23	23
Stratum 3	23	23	25	25
MST00, Males, Quartic of Pscore				
	P-values>.1	P-values>.05	P-values>.01	Regressors
Stratum 1	26	27	28	28
Stratum 2	20	23	23	23
Stratum 3	22	23	25	25
QST00, Females, Cubic of Pscore				
	P-values>.1	P-values>.05	P-values>.01	Regressors
Stratum 1	30	31	31	31
Stratum 2	28	29	30	30
Stratum 3	15	15	15	15
QST00, Females, Quartic of Pscore				
	P-values>.1	P-values>.05	P-values>.01	Regressors
Stratum 1	30	31	31	31
Stratum 2	24	29	30	30
Stratum 3	14	14	15	15
MST00, Females, Cubic of Pscore				
	P-values>.1	P-values>.05	P-values>.01	Regressors
Stratum 1	25	28	30	30
Stratum 2	29	29	30	30
Stratum 3	22	24	25	25
MST00, Females, Quartic of Pscore				
	P-values>.1	P-values>.05	P-values>.01	Regressors
Stratum 1	27	29	30	30
Stratum 2	28	30	30	30
Stratum 3	20	23	25	25

Table 7: Variable Definitions for the 1980–1992 Sample

Label	Definition
Personal Attributes	
aXXYY	Age at start of unemployment $\geq$XX and $\leq$ YY
age	Age at start of unemployment
lnage	log–age at start of unemployment
female	Female
foreign	Non–German citizenship
kids	Has dependent children
married	Married
BIL1	No vocational training degree or information missing
BIL2	Vocational training degree
BIL3	Abitur/No vocational training degree
BIL4	University or technical college degree
BIL5	Missing
Last Employment	
BER1	Apprentice
BER2	Full–time blue collar employee
BER3	Full–time white collar employee
BER4	Working from home with low hours or information missing
BER5	Part–time working
pentg	Daily earnings $\geq$ €15 per day in 1995 €
entgcens	Earnings censored at social security taxation threshold
entg	Daily earnings if pentg=1 and entgcens=0, otherwise zero
logentg	log of entg if pentg=1 and entgcens=0, otherwise zero
claim0	Entitlement to unemployment benefits at beginning of Stratum 1
claim181	Entitlement to unemployment benefits at beginning of Stratum 2
claim361	Entitlement to unemployment benefits at beginning of Stratum 3
lnclaimX	log of claimX if claimX $>$ 0, else zero (X = 0, 181, 361)
claimXg0	claimX=0
claimXg1	claimX>0 and claim0 $\leq$ 170
claimXg2	claimX>170 and claim0 $\leq$ 350
claimXg3	claimX>350
Last Employer	
WZW1	Agriculture
WZW2	Basic materials
WZW3	Metal, vehicles, electronics
WZW4	Light industry
WZW5	Construction
WZW6	Production oriented services, trade, banking
WZW7	Consumer oriented, organizational, and social services, missings

<continued on next page>

Label	Definition
frmsize1	Firm Size (employment) missing or ≤ 10
frmsize2	Firm Size (employment) > 10 and ≤ 200
frmsize3	Firm Size (employment) > 200 and ≤ 500
frmsize4	Firm Size (employment) > 500
Employment and Program History	
preexM	Employed M (M=6, 12, 24) month before unemployment starts
preex6cum	Number of months employed in the last 6 months before unemployment starts
preex12cum	Number of months employed in the last 12 months before unemployment starts
preex24cum	Number of months employed in the last 24 months before unemployment starts
preex60cum	Number of months employed in the last 60 months before unemployment starts
pretxY	Participation in any ALMP program reported in our data in year(s) Y (Y=1, 2) before unemployment starts
Regional Information	
LAND6	Schleswig–Holstein/Hamburg
LAND7	Niedersachsen/Bremen
LAND8	Nordrhein–Westfalen
LAND9	Hessen
LAND10	Rheinland–Pfalz/Saarland
LAND11	Baden–Württemberg
LAND12	Bayern
Calendar Time of Entry into Unemployment	
tnull	First unemployment month (January 1960=0)
y19YY	Unemployment begins in year 19YY
Interaction of Variables	
south	Baden–Württemberg/Bayern
middle	Hessen/Rheinland–Pfalz/Saarland
north	Schleswig–Holstein/Hamburg/Niedersachsen/Bremen
BILXaXXYY	Interactions between education category X and age between XX and YY years
BILXBERY	Interactions between education category X and occupation category Y
yXXYY	Sart of unemployment in year 19XX to 19YY

Notes: All variables except those referring to benefit claims are defined at the time of entry into unemployment and constant during the unemployment spell.

Table 8: Estimated Coefficients of the Propensity Scores for the 1980–1992 Sample

Participation Probit for ST8092, Males

	Stratum 1	Stratum 2	Stratum 3
BER2		-0.201 (0.186)	0.123 (0.072)*
BER3	0.229 (0.072)***		
BIL1BER2	0.114 (0.072)	0.365 (0.242)	
BIL1BER3		0.636 (0.288)**	
BIL1a3034		0.341 (0.144)**	
BIL1a3544		-0.511 (0.252)**	
BIL2		0.429 (0.222)*	
BIL2BER3		-0.068 (0.192)	
BIL2a3544		-0.416 (0.223)*	
BIL4	-0.188 (0.123)		
LAND10	0.286 (0.086)***		
LAND8	0.169 (0.061)***		
LAND9	0.262 (0.084)***		
WZW1	0.235 (0.146)		
WZW2	0.231 (0.098)**		
WZW3	0.301 (0.086)***	0.177 (0.073)**	
WZW6	0.184 (0.080)**	0.125 (0.060)**	
WZW7	0.121 (0.095)		
a3044			0.161 (0.058)***
a3544		0.520 (0.217)**	
a4553	-0.183 (0.076)**		
claim0	-0.002 (0.001)*		
claim0g0	-0.466 (0.353)		
claim0g1	-0.479 (0.268)*		
claim0g2	-0.207 (0.130)		
claim181		-0.001 (0.000)***	
entgcens	-0.127 (0.179)		-0.254 (0.234)
foreign	-0.145 (0.085)*	-0.130 (0.083)	-0.284 (0.090)***
frmsize1	-0.310 (0.088)***		-0.025 (0.065)
frmsize2	-0.174 (0.078)**		
frmsize3	-0.049 (0.103)		0.122 (0.092)
kids	-0.134 (0.077)*		
lnage		-0.112 (0.126)	0.123 (0.133)
lnclaim361			-0.028 (0.014)**
logentg	-0.030 (0.023)	0.036 (0.030)	-0.043 (0.040)
married	-0.003 (0.069)		-0.030 (0.062)
middle		-0.012 (0.073)	0.147 (0.075)*
north		-0.151 (0.071)**	-0.184 (0.079)**
preex12cum	0.019 (0.019)		-0.043 (0.022)*
preex24cum	-0.019 (0.011)*	0.001 (0.007)	0.018 (0.008)**
preex60cum	0.001 (0.003)	0.003 (0.002)	
preex6cum			0.036 (0.049)
south		-0.147 (0.071)**	-0.182 (0.079)**
tnull		-0.001 (0.001)	
y1982		-0.222 (0.094)**	
y1983		-0.265 (0.099)***	
y1987		-0.254 (0.114)**	
y1990			0.150 (0.104)
y1991			0.385 (0.196)**
y8182	0.175 (0.069)**		
y8687	0.361 (0.060)***		
intercept	-2.159 (0.405)***	-2.130 (0.507)***	-2.676 (0.533)***
N	60083	25711	15814

<continued on next page>

Table 8: Estimated Coefficients of the Propensity Scores for the 1980–1992 Sample

Participation Probit for ST8092, Females

	Stratum 1	Stratum 2	Stratum 3
BER2			0.024 (0.088)
BER3			0.163 (0.083)*
BIL1	0.477 (0.172)***		
BIL2	0.443 (0.180)**		
BIL2a3034	-0.148 (0.150)		
BIL2a3544	0.171 (0.086)**		
LAND11		-0.181 (0.110)*	
LAND8	0.187 (0.060)***		
WZW3	0.094 (0.085)	-0.141 (0.114)	
WZW6		0.029 (0.077)	
WZW7		-0.170 (0.087)**	
a3034	0.322 (0.123)***		
a3544		0.123 (0.069)*	0.124 (0.067)*
claim0g1	0.578 (0.356)		
claim0g2	0.861 (0.449)*		
claim0g3	0.825 (0.482)*		
claim181g0		0.933 (0.308)***	
claim181g1		0.772 (0.183)***	
claim181g2		0.573 (0.146)***	
claim_361			-0.219 (0.088)**
entgcens			0.370 (0.298)
foreign	-0.277 (0.121)**		
frmsize1	-0.119 (0.064)*		-0.180 (0.094)*
frmsize2			-0.208 (0.090)**
frmsize4			-0.046 (0.100)
kids	-0.036 (0.081)	0.005 (0.084)	0.025 (0.087)
lnage	0.036 (0.140)	-0.155 (0.150)	
lnclaim0	-0.133 (0.080)*		
lnclaim181		0.050 (0.044)	
logentg	0.087 (0.036)**	0.088 (0.042)**	0.045 (0.047)
married	-0.128 (0.058)**	-0.125 (0.061)**	-0.150 (0.059)**
middle		0.133 (0.080)*	
north		0.022 (0.076)	-0.108 (0.075)
preex12		-0.111 (0.084)	0.276 (0.135)**
preex12cum			-0.061 (0.033)*
preex24		0.174 (0.078)**	
preex24cum			0.005 (0.011)
preex6	-0.145 (0.082)*		0.146 (0.189)
preex60cum			0.003 (0.003)
pretx2			0.317 (0.147)**
south			-0.265 (0.075)***
tnull	0 (0.001)	0.002 (0.001)**	0 (0.001)
y1984		-0.256 (0.130)**	
y1986	0.294 (0.084)***		
y1990	0.130 (0.117)		
y1991	0.487 (0.178)***		
y8486			0.255 (0.069)***
y8889		-0.245 (0.099)**	0.275 (0.102)***
intercept	-3.572 (0.576)***	-3.530 (0.609)***	-2.336 (0.389)***
N	35927	23115	17148

Table 9: Results of Smith and Todd (2005) Balancing Test for the 1980–1992 Sample

ST8092, Males, Cubic of Pscore				
	P-values>.1	P-values>.05	P-values>.01	Regressors
Stratum 1	27	29	29	29
Stratum 2	22	23	24	24
Stratum 3	18	18	18	18
ST8092, Males, Quartic of Pscore				
	P-values>.1	P-values>.05	P-values>.01	Regressors
Stratum 1	22	26	29	29
Stratum 2	21	24	24	24
Stratum 3	15	18	18	18

ST8092, Females, Cubic of Pscore				
	P-values>.1	P-values>.05	P-values>.01	Regressors
Stratum 1	22	22	22	22
Stratum 2	20	20	20	20
Stratum 3	21	22	22	22
ST8092, Females, Quartic of Pscore				
	P-values>.1	P-values>.05	P-values>.01	Regressors
Stratum 1	17	19	21	22
Stratum 2	18	18	20	20
Stratum 3	20	22	22	22

Table 10: Estimated Coefficients for Test of Heterogeneity of Employment Effects over Time (Men, Including Lock–In Period)

	Stratum 1	Stratum 2	Stratum 3
Year 1980[a]	-.046 (.150)	.164 (.212)	
Year 1981	.020 (.131)	-.114 (.075)	.104 (.204)
Year 1982	.065 (.116)	.045 (.104)	.096 (.160)
Year 1983	.056 (.107)	.196 (.105)	.011 (.135)
Year 1984	.098 (.140)	.084 (.102)	.103 (.121)
Year 1985	.258 (.168)	.121 (.090)	.096 (.155)
Year 1986	.204 (.076)	.029 (.087)	.003 (.141)
Year 1987	.147 (.084)	.122 (.099)	.136 (.147)
Year 1988	.016 (.139)	-.074 (.128)	-.123 (.172)
Year 1989	.160 (.124)	.169 (.122)	.155 (.213)
Year 1990	-.015 (.134)	.017 (.136)	.107 (.179)
Year 1991	.065 (.261)	-.041 (.129)	-.052 (.150)
Year 1992			.217 (.168)
Month 1[b]	-.135 (.092)		
Month 2	-.119 (.091)		
Month 3	-.066 (.094)		
Month 4	-.058 (.105)		
Month 6		.082 (.084)	
Month 7		-.033 (.086)	
Month 8		-.030 (.086)	
Month 9		-.018 (.086)	
Month 10		-.023 (.092)	
Month 12			.017 (.142)
Month 13			-.018 (.142)
Month 14			-.182 (.159)
Month 15			-.105 (.150)
Month 16			-.043 (.141)
Month 17			.207 (.184)
Month 18			-.006 (.175)
Month 20			-.028 (.168)
Month 21			-.072 (.169)
Month 22			-.100 (.160)
Month 23			-.128 (.142)

Notes: Regression of the individual treatment effects averaged over the months following program start on year dummies and elapsed unemployment duration at program start. Empirical standard errors in parentheses are calculated from bootstrap resamples. Lock–in period: until month six since program start, end of the observation period: month 48 since program start. [a] 'Year XXXX' denotes the year of program start. [b] 'Month Y' denotes the month of elapsed unemployment duration at program start. Omitted categories: month 5 (stratum 1), month 11 (stratum 2), month 19 (stratum 3).

Table 11: Estimated Coefficients for Test of Heterogeneity of Employment Effects over Time (Women, Including Lock–In Period)

	Stratum 1	Stratum 2	Stratum 3
Year 1980[a]	-.003 (.195)		
Year 1981	.144 (.117)	-.088 (.183)	-.076 (.116)
Year 1982	.012 (.104)	.058 (.143)	-.020 (.166)
Year 1983	.132 (.119)	-.001 (.118)	.016 (.107)
Year 1984	.113 (.170)	.276 (.192)	.136 (.096)
Year 1985	.170 (.155)	.144 (.166)	.063 (.107)
Year 1986	.065 (.111)	-.061 (.142)	.090 (.091)
Year 1987	.051 (.103)	.101 (.122)	-.037 (.102)
Year 1988	.051 (.157)	-.091 (.138)	-.038 (.154)
Year 1989	.043 (.145)	-.064 (.149)	-.176 (.117)
Year 1990	.062 (.122)	-.050 (.151)	-.006 (.088)
Year 1991	.111 (.142)	-.058 (.127)	-.083 (.120)
Year 1992			-.148 (.153)
Month 1[b]	-.045 (.129)		
Month 2	-.112 (.097)		
Month 3	-.041 (.100)		
Month 5	-.095 (.098)		
Month 6		-.020 (.122)	
Month 7		.099 (.121)	
Month 8		.205 (.131)	
Month 9		-.077 (.129)	
Month 10		-.009 (.145)	
Month 12			-.040 (.100)
Month 13			.124 (.128)
Month 14			-.042 (.093)
Month 15			.228 (.137)
Month 16			.125 (.119)
Month 18			-.012 (.093)
Month 19			.100 (.141)
Month 20			.264 (.155)
Month 21			-.003 (.144)
Month 22			.270 (.109)
Month 23			-.043 (.086)

Notes: Regression of the individual treatment effects averaged over the months following program start on year dummies and elapsed unemployment duration at program start. Empirical standard errors in parentheses are calculated from bootstrap resamples. Lock–in period: until month six since program start, end of the observation period: month 48 since program start. [a] 'Year XXXX' denotes the year of program start. [b] 'Month Y' denotes the month of elapsed unemployment duration at program start. Omitted categories: month 4 (stratum 1), month 11 (stratum 2), month 17 (stratum 3).

Table 12: Estimated Coefficients for Test of Heterogeneity of Employment Effects over Time (Men, After Lock–In Period)

	Stratum 1	Stratum 2	Stratum 3
Year 1980[a]	-.039 (.169)	.188 (.229)	
Year 1981	.035 (.145)	-.118 (.082)	.124 (.224)
Year 1982	.075 (.127)	.046 (.111)	.090 (.175)
Year 1983	.063 (.116)	.235 (.115)	.008 (.147)
Year 1984	.112 (.157)	097 (.111)	.123 (.135)
Year 1985	.283 (.180)	.128 (.096)	.106 (.168)
Year 1986	.230 (.085)	.039 (.096)	.008 (.154)
Year 1987	.173 (.093)	.132 (.111)	.145 (.162)
Year 1988	.039 (.151)	-.067 (.141)	-.143 (.186)
Year 1989	.175 (.130)	.195 (.134)	.173 (.234)
Year 1990	-.010 (.148)	.038 (.150)	.120 (.196)
Year 1991	.074 (.276)	-.043 (.142)	-.072 (.166)
Year 1992			.237 (.187)
Month 1[b]	-.146 (.101)		
Month 2	-.115 (.101)		
Month 3	-.081 (.104)		
Month 4	-.048 (.116)		
Month 6		.099 (.092)	
Month 7		-.038 (.094)	
Month 8		-.030 (.095)	
Month 9		-.009 (.095)	
Month 10		-.025 (.101)	
Month 12			.026 (.155)
Month 13			-.008 (.155)
Month 14			-.201 (.173)
Month 15			-.108 (.165)
Month 16			-.045 (.154)
Month 17			.228 (.198)
Month 18			-.012 (.191)
Month 20			-.021 (.185)
Month 21			-.067 (.189)
Month 22			-.113 (.174)
Month 23			-.144 (.156)

Notes: Regression of the individual treatment effects averaged over the months following program start on year dummies and elapsed unemployment duration at program start. Empirical standard errors in parentheses are calculated from bootstrap resamples. Lock–in period: until month six since program start, end of the observation period: month 48 since program start. [a] 'Year XXXX' denotes the year of program start. [b] 'Month Y' denotes the month of elapsed unemployment duration at program start. Omitted categories: month 5 (stratum 1), month 11 (stratum 2), month 19 (stratum 3).

Table 13: Estimated Coefficients for Test of Heterogeneity of Employment Effects over Time (Women, After Lock–In Period)

	Stratum 1	Stratum 2	Stratum 3
Year 1980[a]	.009 (.216)		
Year 1981	.143 (.124)	-.093 (.198)	-.085 (.126)
Year 1982	.011 (.115)	.055 (.156)	-.030 (.183)
Year 1983	.146 (.128)	.008 (.129)	.021 (.118)
Year 1984	.130 (.183)	.320 (.213)	.146 (.107)
Year 1985	.185 (.169)	.161 (.182)	.076 (.119)
Year 1986	.079 (.120)	-.065 (.156)	.100 (.100)
Year 1987	.057 (.112)	.116 (.135)	-.046 (.109)
Year 1988	.024 (.168)	-.093 (.153)	-.030 (.174)
Year 1989	.055 (.160)	-.068 (.165)	-.184 (.128)
Year 1990	.079 (.131)	-.053 (.165)	-.010 (.099)
Year 1991	.130 (.153)	-.044 (.139)	-.088 (.129)
Year 1992			-.166 (.167)
Month 1[b]	-.032 (.139)		
Month 2	-.118 (.104)		
Month 3	-.047 (.109)		
Month 5	-.098 (.106)		
Month 6		-.015 (.134)	
Month 7		.116 (.134)	
Month 8		.234 (.143)	
Month 9		-.081 (.143)	
Month 10		-.014 (.160)	
Month 12			-.034 (.112)
Month 13			.133 (.141)
Month 14			-.046 (.102)
Month 15			.235 (.146)
Month 16			.136 (.127)
Month 18			-.021 (.103)
Month 19			.115 (.157)
Month 20			.285 (.174)
Month 21			.004 (.162)
Month 22			.295 (.122)
Month 23			-.054 (.093)

Notes: Regression of the individual treatment effects averaged over the months following program start on year dummies and elapsed unemployment duration at program start. Empirical standard errors in parentheses are calculated from bootstrap resamples. Lock–in period: until month six since program start, end of the observation period: month 48 since program start. [a] 'Year XXXX' denotes the year of program start. [b] 'Month Y' denotes the month of elapsed unemployment duration at program start. Omitted categories: month 4 (stratum 1), month 11 (stratum 2), month 17 (stratum 3).

Table 14: Estimated Coefficients for Test of Heterogeneity of Employment Effects over Time (Men, During Lock–In Period)

	Stratum 1	Stratum 2	Stratum 3
Year 1980[a]	-.095 (.073)	-.013 (.152)	
Year 1981	-.088 (.095)	-.080 (.056)	-.040 (.133)
Year 1982	-.005 (.085)	.037 (.088)	.139 (.132)
Year 1983	.010 (.103)	-.080 (.074)	.032 (.109)
Year 1984	-.004 (.093)	-.007 (.090)	-.037 (.097)
Year 1985	.081 (.126)	.073 (.085)	.030 (.128)
Year 1986	.019 (.078)	-.037 (.067)	-.032 (.125)
Year 1987	-.040 (.082)	.049 (.073)	.069 (.117)
Year 1988	-.149 (.105)	-.126 (.098)	.018 (.132)
Year 1989	.056 (.146)	-.015 (.094)	.022 (.153)
Year 1990	-.054 (.120)	-.136 (.088)	.020 (.125)
Year 1991	-.003 (.192)	-.025 (.095)	.092 (.124)
Year 1992			.074 (.130)
Month 1[b]	-.053 (.085)		
Month 2	-.142 (.074)		
Month 3	.039 (.081)		
Month 4	-.136 (.075)		
Month 6		-.038 (.074)	
Month 7		.000 (.066)	
Month 8		-.032 (.068)	
Month 9		-.078 (.068)	
Month 10		-.010 (.071)	
Month 12			-.046 (.121)
Month 13			-.086 (.116)
Month 14			-.042 (.127)
Month 15			-.087 (.120)
Month 16			-.034 (.119)
Month 17			.057 (.151)
Month 18			.037 (.142)
Month 20			-.077 (.128)
Month 21			-.109 (.115)
Month 22			-.005 (.124)
Month 23			-.013 (.115)

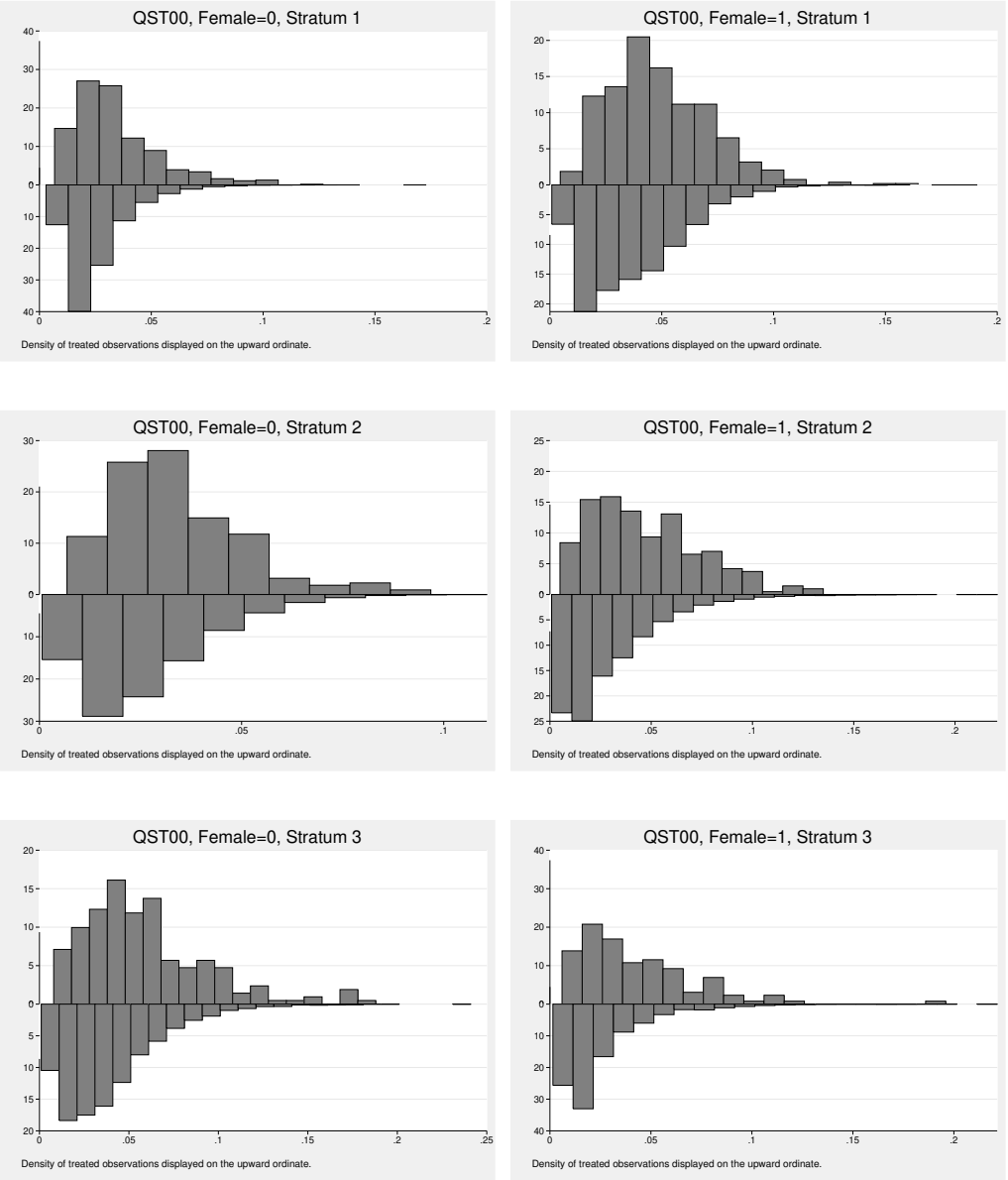
Notes: Regression of the individual treatment effects averaged over the months following program start on year dummies and elapsed unemployment duration at program start. Empirical standard errors in parentheses are calculated from bootstrap resamples. Lock–in period: until month six since program start, end of the observation period: month 48 since program start. [a] 'Year XXXX' denotes the year of program start. [b] 'Month Y' denotes the month of elapsed unemployment duration at program start. Omitted categories: month 5 (stratum 1), month 11 (stratum 2), month 19 (stratum 3).

Table 15: Estimated Coefficients for Test of Heterogeneity of Employment Effects over Time (Women, During Lock–In Period)

	Stratum 1	Stratum 2	Stratum 3
Year 1980[a]	-.095 (.091)		
Year 1981	.147 (.104)	-.051 (.127)	-.015 (.097)
Year 1982	.017 (.089)	.079 (.103)	.055 (.138)
Year 1983	.032 (.091)	-.061 (.076)	-.018 (.092)
Year 1984	-.008 (.123)	-.040 (.114)	.060 (.090)
Year 1985	.057 (.130)	.023 (.127)	-.030 (.059)
Year 1986	-.039 (.080)	-.032 (.091)	.018 (.075)
Year 1987	.007 (.083)	-.005 (.092)	.023 (.094)
Year 1988	.243 (.140)	-.078 (.081)	-.097 (.083)
Year 1989	-.044 (.107)	-.033 (.093)	-.112 (.086)
Year 1990	-.062 (.087)	-.027 (.093)	.020 (.072)
Year 1991	-.029 (.124)	-.161 (.104)	-.051 (.102)
Year 1992			-.020 (.119)
Month 1[b]	-.138 (.101)		
Month 2	-.064 (.082)		
Month 3	.001 (.082)		
Month 5	-.072 (.079)		
Month 6		-.058 (.078)	
Month 7		-.022 (.088)	
Month 8		-.006 (.093)	
Month 9		-.056 (.095)	
Month 10		.029 (.087)	
Month 12			-.083 (.070)
Month 13			.057 (.101)
Month 14			-.014 (.080)
Month 15			.180 (.123)
Month 16			.045 (.097)
Month 18			.051 (.083)
Month 19			-.014 (.076)
Month 20			.118 (.132)
Month 21			-.053 (.069)
Month 22			.093 (.106)
Month 23			.036 (.090)

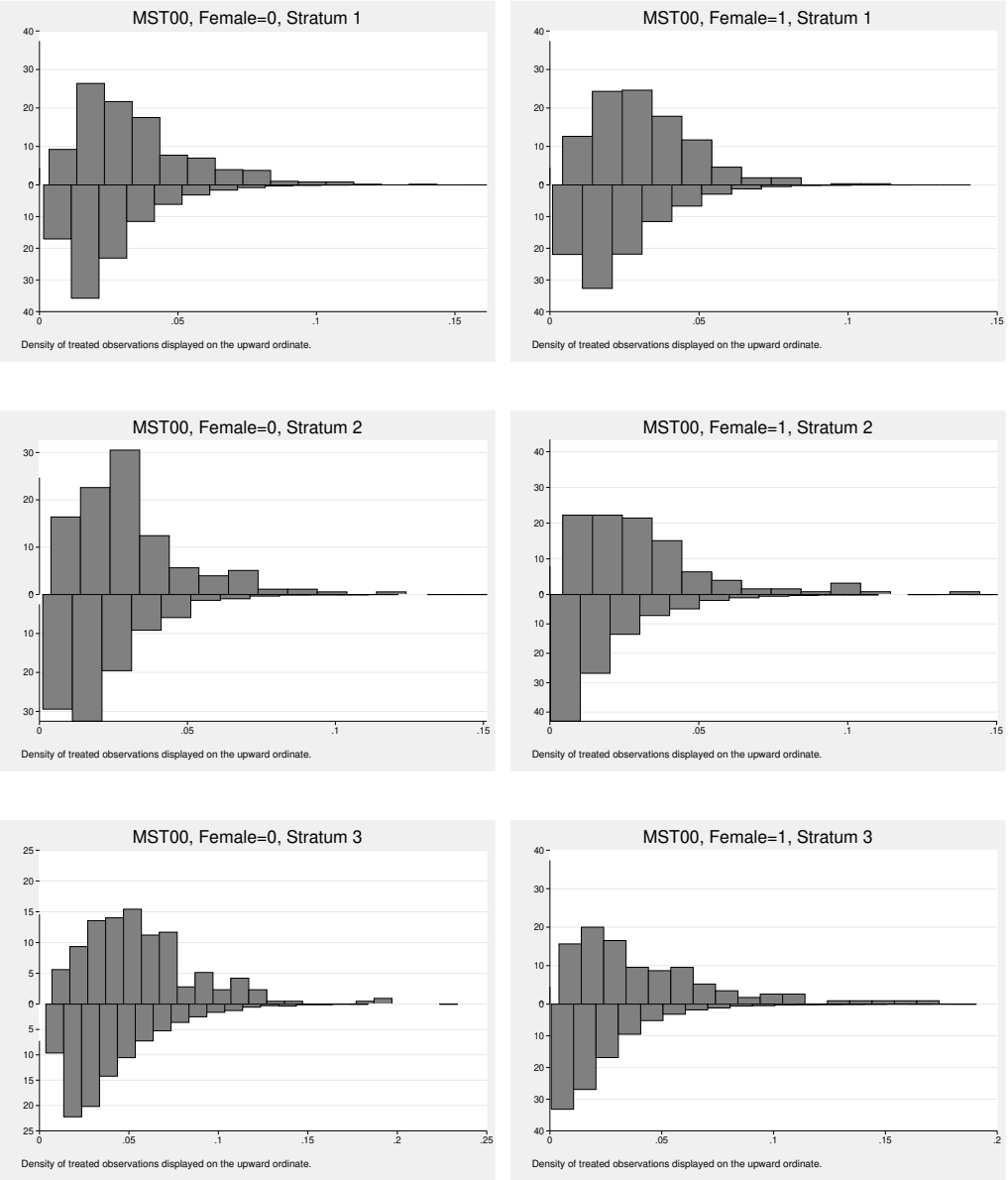
Notes: Regression of the individual treatment effects averaged over the months following program start on year dummies and elapsed unemployment duration at program start. Empirical standard errors in parentheses are calculated from bootstrap resamples. Lock–in period: until month six since program start, end of the observation period: month 48 since program start. [a] 'Year XXXX' denotes the year of program start. [b] 'Month Y' denotes the month of elapsed unemployment duration at program start. Omitted categories: month 4 (stratum 1), month 11 (stratum 2), month 17 (stratum 3).

Figure 1: Graphical Check of Common Support for QST00



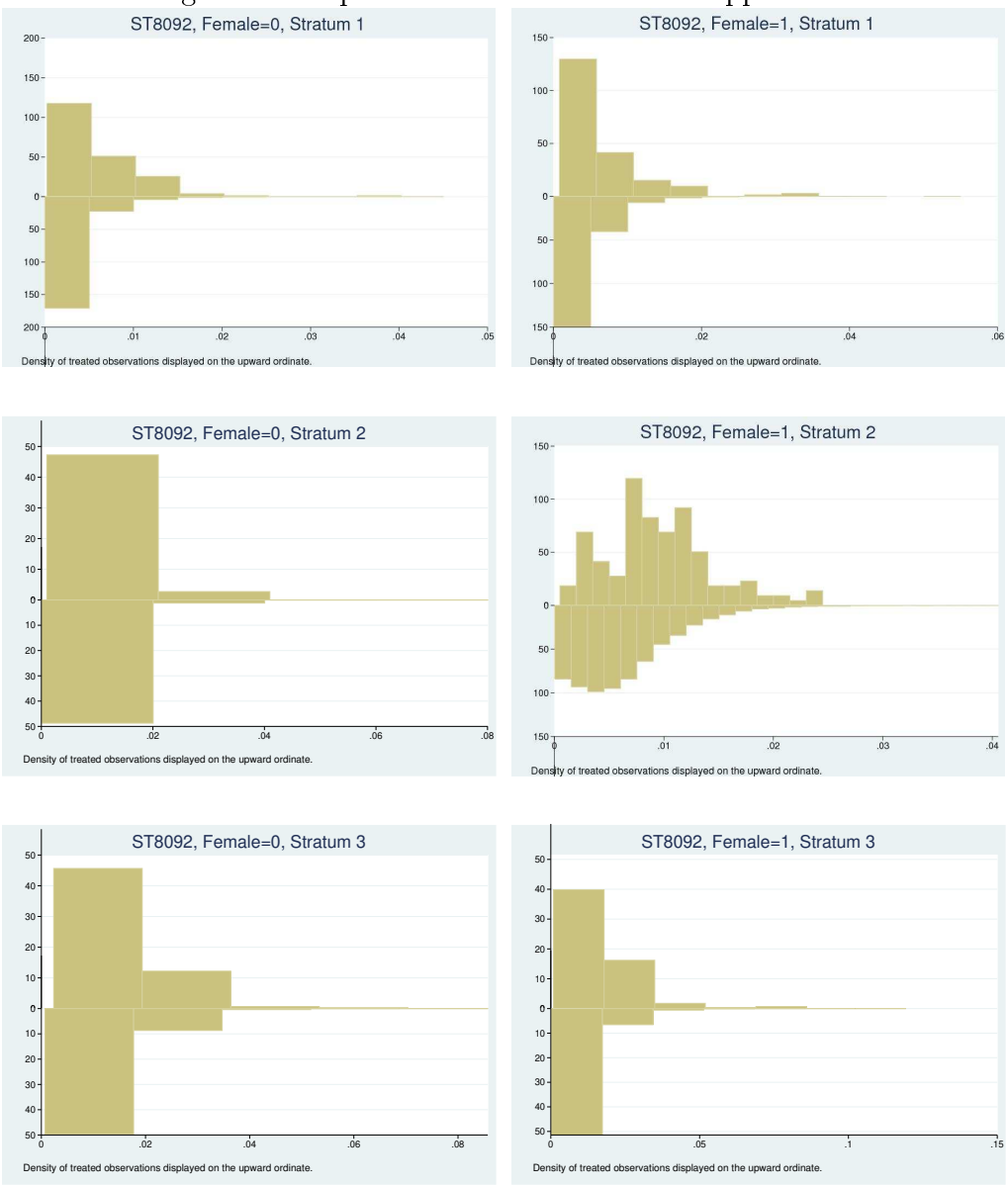
Notes: QST00 refers to the qualification variant of short–term training administered in the early 2000s.

Figure 2: Graphical Check of Common Support for MST00



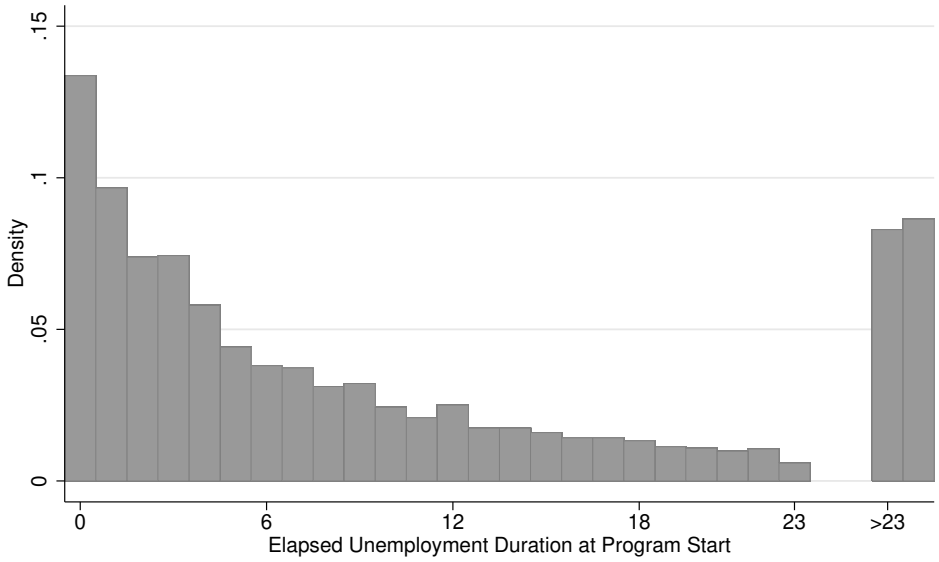
Notes: MST00 refers to the monitoring variant of short–term training administered in the early 2000s.

Figure 3: Graphical Check of Common Support for ST8092



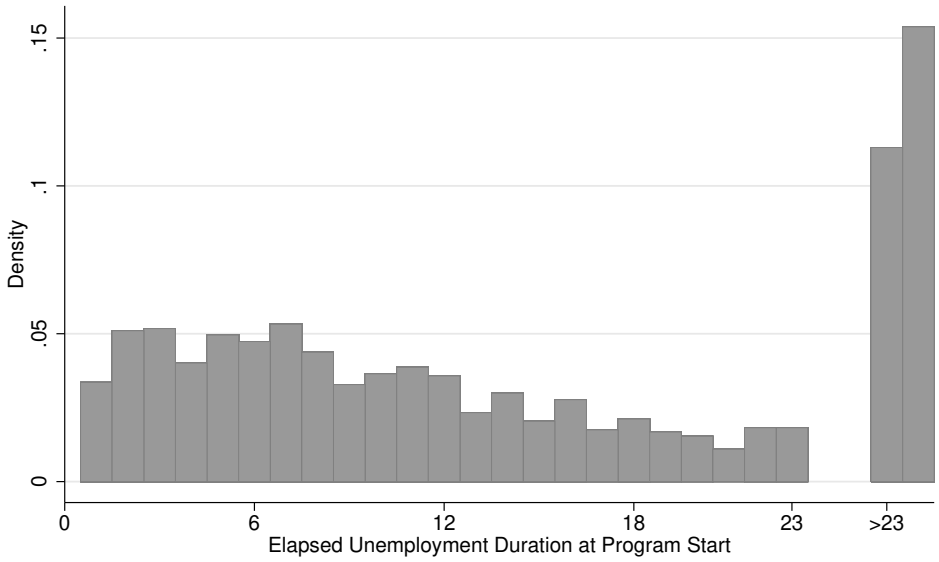
Notes: ST8092 refers to short–term training administered between 1980 and 1992.

Figure 4: Elapsed Unemployment Duration at Start of Short–Term Training in the Early 2000s (ST00)



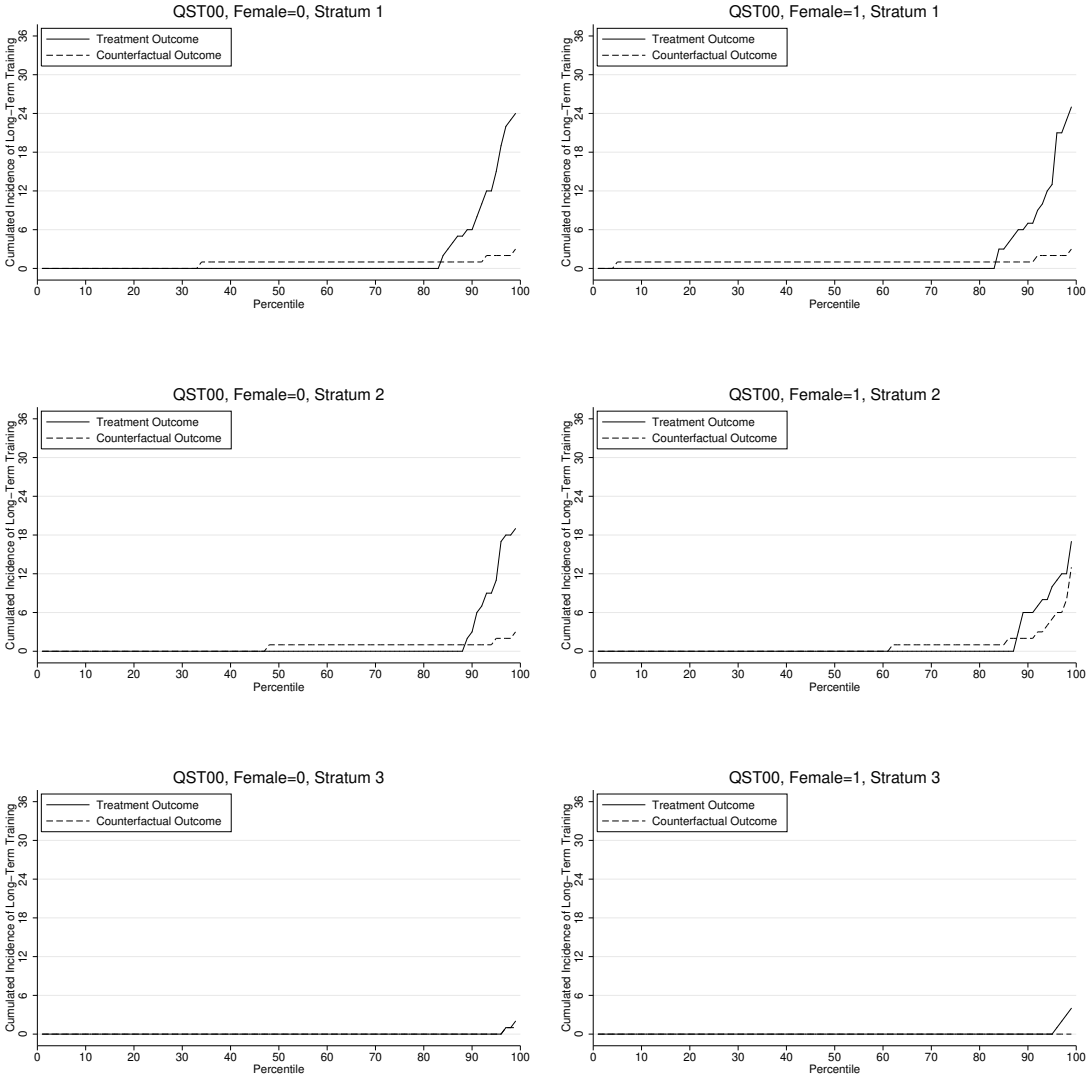
Notes: Elapsed unemployment durations above 23 months are divided into two categories: The first bar shows the density of unemployment durations between 24 and 35 months, the second bar refers to the density of unemployment durations larger than 35 months.

Figure 5: Elapsed Unemployment Duration at Start of Short–Term Training Between 1980 and 1992 (ST8092)



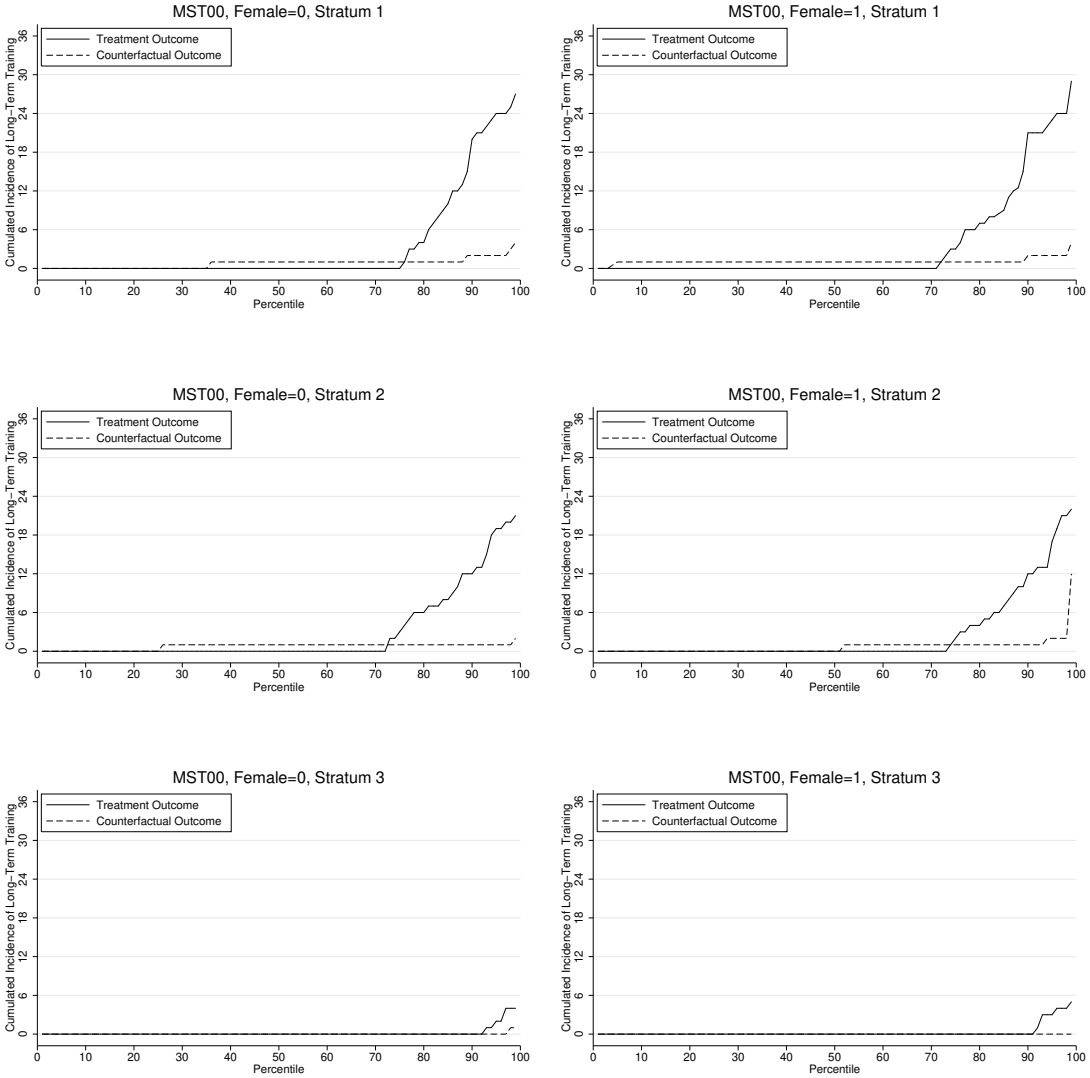
Notes: Elapsed unemployment durations above 23 months are divided into two categories: The first bar shows the density of unemployment durations between 24 and 35 months, the second bar refers to the density of unemployment durations larger than 35 months.

Figure 6: Percentiles of Cumulated Incidence of Long–Term Training for QST00 Participants and Matched Comparisons



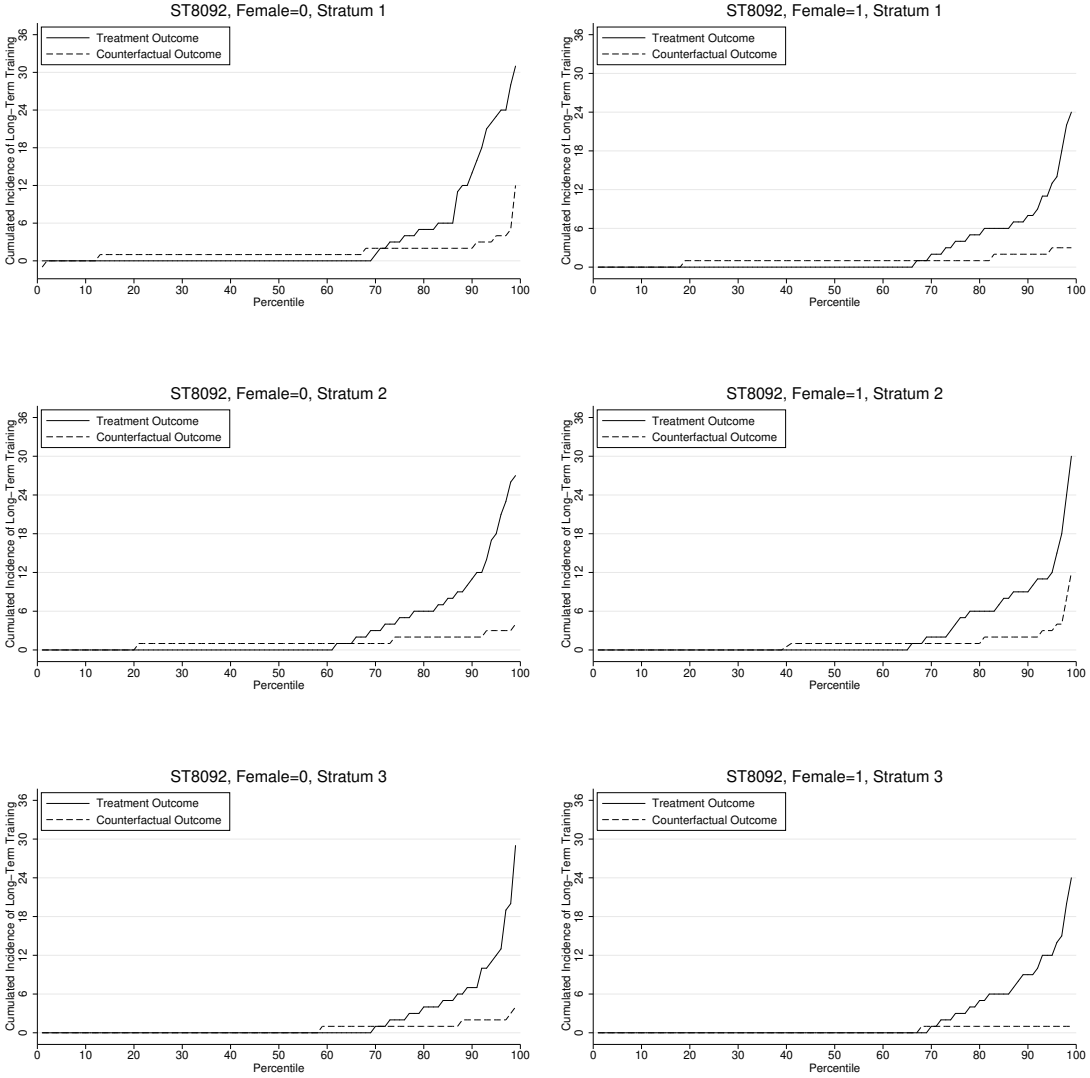
Notes: QST00 refers to the qualification variant of short–term training administered in the early 2000s. Cumulated incidence of long–term training calculated as the sum of the monthly participation dummies from the beginning of unemployment until the end of the observation period, i.e. over 36 months in the first stratum, 30 months in the second, and 18 months in the third.

Figure 7: Percentiles of Cumulated Incidence of Long–Term Training for MST00 Participants and Matched Comparisons



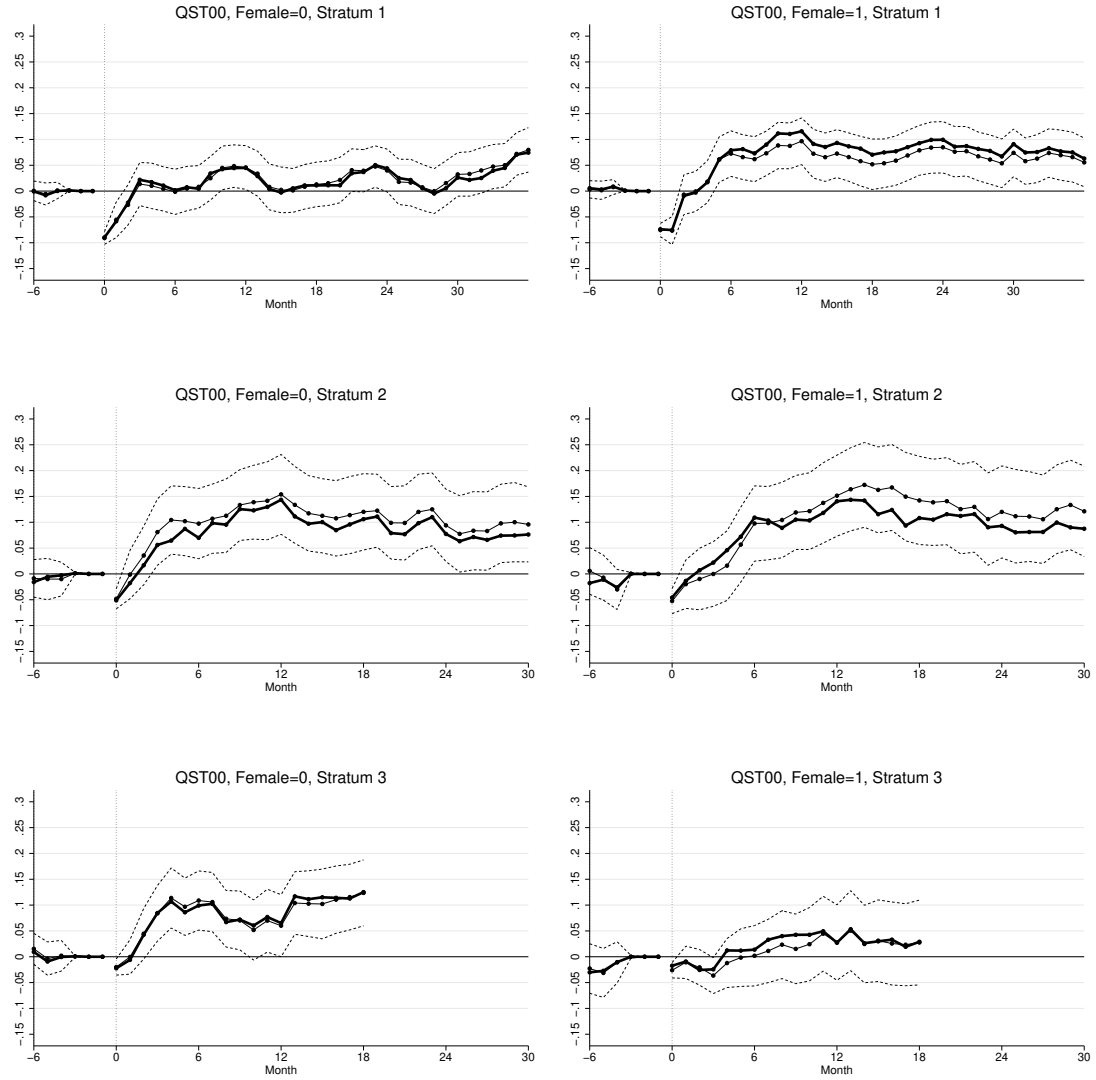
Notes: MST00 refers to the monitoring variant of short–term training administered in the early 2000s. Cumulated incidence of long–term training calculated as the sum of the monthly participation dummies from the beginning of unemployment until the end of the observation period, i.e. over 36 months in the first stratum, 30 months in the second, and 18 months in the third.

Figure 8: Percentiles of Cumulated Incidence of Long–Term Training for ST8092 Participants and Matched Comparisons



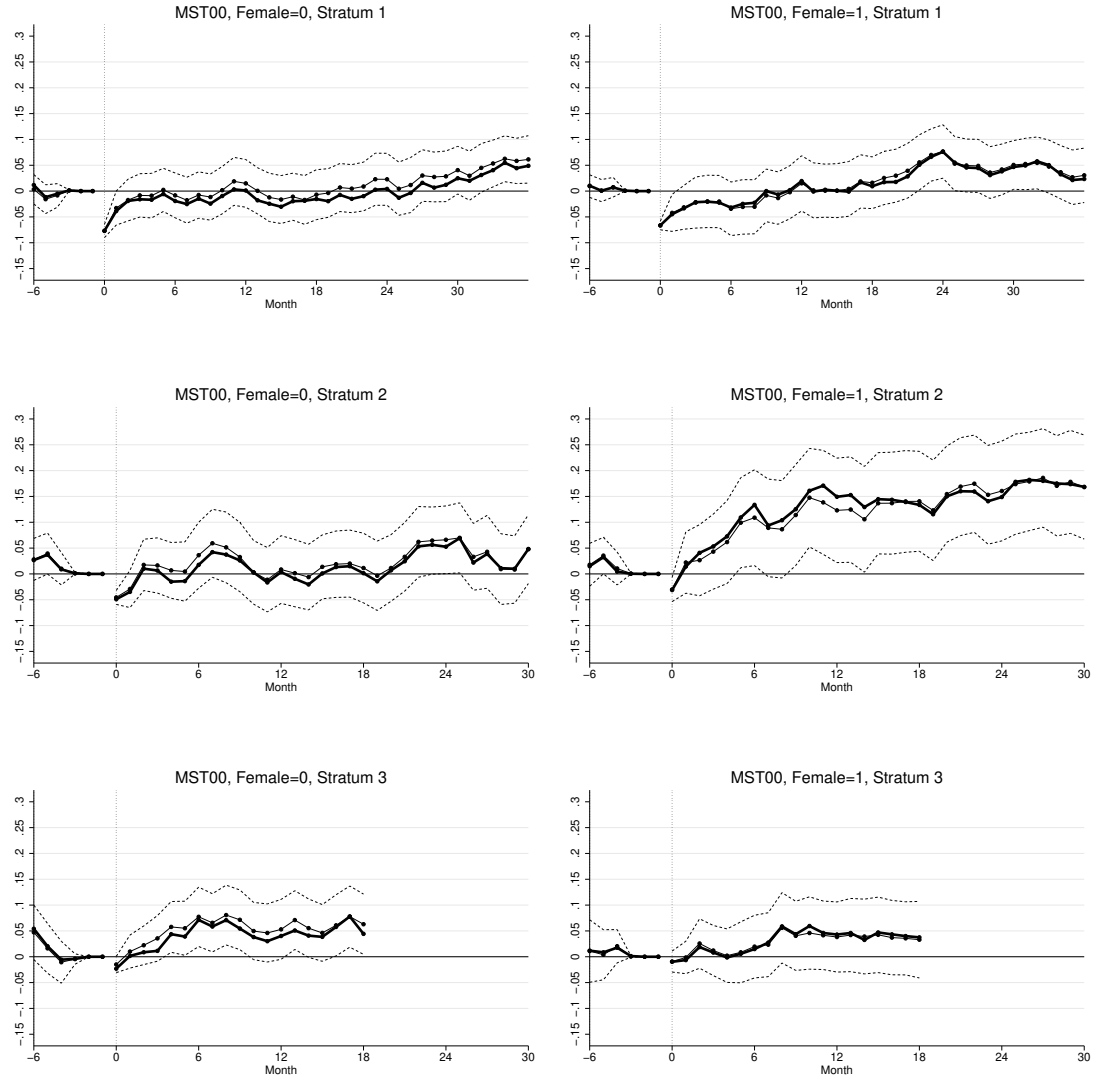
Notes: ST8092 refers to short–term training administered between 1980 and 1992. Cumulated incidence of long–term training calculated as the sum of the monthly participation dummies from the beginning of unemployment until the end of the observation period, i.e. over 66 months in the first stratum, 60 months in the second, and 48 months in the third.

Figure 9: Comparison of Average Treatment Effect on the Treated for QST00 on Employment in Benchmark Specification to Reduced Specification



Notes: QST00 refers to the qualification variant of short–term training administered in the early 2000s. The reduced specification only considers the information in the more recent data that is also available in the older data. Difference in employment rates is measured on the ordinate, pre-unemployment (< 0) and post-treatment (≥ 0) months on the abscissa. Lighter line and dashed lines for 95%-confidence intervals refer to the benchmark specification as shown in figure 1 of the paper. Bold line refers to the reduced propensity score specification.

Figure 10: Comparison of Average Treatment Effect on the Treated for MST00 on Employment in Benchmark Specification to Reduced Specification



Notes: MST00 refers to the monitoring variant of short–term training administered in the early 2000s. The reduced specification only considers the information in the more recent data that is also available in the older data. Difference in employment rates is measured on the ordinate, pre-unemployment (< 0) and post-treatment (≥ 0) months on the abscissa. Lighter line and dashed lines for 95%-confidence intervals refer to the benchmark specification as shown in figure 2 of the paper. Bold line refers to the reduced propensity score specification.